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STRUCTURAL PETROLOGY OF PLANES OF LIQUID INCLUSIONS[1]

O. F. TUTTLE

Geophysical Laboratory, Carnegie Institution of Washington

ABSTRACT

Planes of liquid inclusions are common in the quartz of igneous and metamorphic rocks. The planes have a high degree of preferred orientation, and it is believed that they represent fractures in the quartz grains that have been filled with intergranular liquid, which subsequently changed to individual inclusions by differential solution and deposition of silica. The planes of inclusions commonly extend through several quartz grains of different orientation with no deflection at the grain boundaries, indicating that there is little or no crystallographic control of their orientation. They have been found to have a remarkably uniform orientation over an area of 15 × 20 miles.

Detailed work on drag folds indicates that the planes of inclusions may develop in the shear directions of the deforming stress pattern and that one set tends to predominate. The poles to the planes tend to lie in a partial girdle about the fold axis. Some evidence was found indicating that the planes may develop in the tensional direction.

Quartz, then, may fail by shear or tensional rupture and in certain cases by deformation, which produces lamellae that may be a type of translation-gliding. Failure by rupture along crystallographic directions has been described by one investigator and has been produced experimentally in the laboratory. These different phenomena are believed to represent failure under varying environmental conditions, and the optimum development of any type of failure depends on such factors as confining pressure, temperature, composition, and the presence (and character) of solutions.

If several sets of planes of liquid inclusions develop at different times, the relative ages of the planes can be discerned from the character of the planes. Planes of inclusions that are very "young" appear as extremely thin sheets of liquid, whereas "old" planes are made up of inclusions that are nearly equant, with some of the inclusions having crystal planes as boundaries (negative crystals).

INTRODUCTION

GENERAL

Liquid inclusions have been recognized in the quartz grains of rocks since the advent of the microscope, and many observers have noted that they are commonly aligned in planes. The writer recently became interested in these structures when studying liquid inclusions in granitic rocks and associated quartz veins. Examination of oriented sections of a granitic rock from the Washington, D.C., area revealed a remarkable parallelism of the planes. Further observations on crystalline rocks in general showed that planes of liquid inclusions are very common; indeed, it is unusual to find quartz-bearing rocks that do not contain them. A statistical study of the orientation of the planes of liquid inclusions made by using the universal stage disclosed a remarkably high degree of orientation.

A cursory examination of the literature

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left much to be desired in the way of explanation of the origin and significance of the planes of inclusions. Several conflicting theories had been advanced for their origin, and the high degree of orientation was not mentioned. It was at this stage that the writer decided to make an extensive study of the inclusions. The following report deals with those studies, which began in 1941 and continued until the summer of 1942, when full-time work was temporarily discontinued. However, laboratory work was continued intermittently, much of it at the Geophysical Laboratory, until 1946, when the present work was completed. Field studies were commenced and were continued, along with the laboratory studies, during parts of 1943, 1944, and 1945.

CHARACTER OF LIQUID INCLUSIONS

Liquid inclusions may be divided into two genetic groups: (1) primary and (2) secondary. Primary inclusions are emplaced during the growth of the host crystal, whereas secondary inclusions are introduced at some subsequent time. Only the latter will be considered here, as primary inclusions are usually randomly distributed throughout a crystal, or, if aligned in planes, the planes represent growth directions (zoning or lineage [Buerger, 1932]) in the crystal.

Liquid inclusions in quartz crystals vary in size from submicroscopic to cavities containing more than 50 cc. of solution. The large inclusions are, in all probability, primary inclusions emplaced during growth. The secondary inclusions which are considered in detail here rarely contain as much as 0.001 cc. of solution. They vary in size from submicroscopic up to a few hundredths of a millimeter in dimensions.

The shape of liquid inclusions is extremely variable. There are all gradations in shape from thin sheetlike inclusions with extremely irregular outlines to nearly equant inclusions bounded by crystal planes characteristic of the host crystal. Between these two extremes round, elliptical, cylindrical, cigar-shaped, pear-shaped, or irregular branching forms are found. Secondary inclusions usually have two dimensions considerably greater than the third. The inclusions usually lie in a plane with the planar element of the inclusions parallel to the plane.

Liquid inclusions are characterized by a vapor bubble which formed on cooling as a result of the much higher coefficient of expansion of the liquid than of the host crystal. In comparison with the liquid, the bubble is usually small, and it is not unusual to find it in constant motion (Brownian movement). This mobility of the bubble serves to distinguish liquid inclusions from glass inclusions found in some extrusive rocks.

If the fluid escapes from the inclusions (as always happens to those inclusions cut by the surfaces of the thin section), the liquid is replaced by air, which has a much lower index of refraction than has

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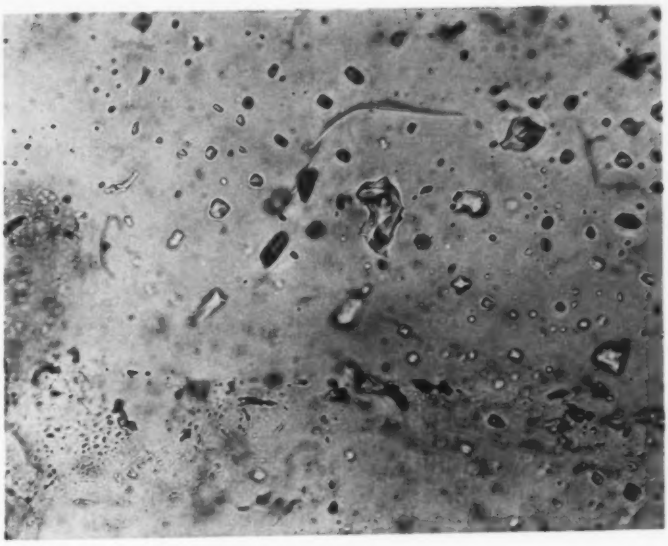
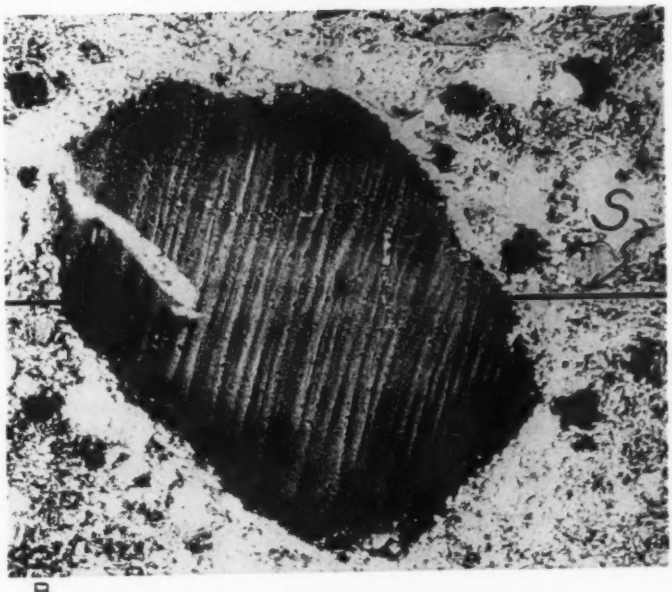
PLATE 1

A, Plane of liquid inclusions in quartz grain. The plane is nearly perpendicular to the microscope axis. The inclusions in the upper left-hand part of the photograph have boundaries that suggest imperfect negative crystal faces. ×500.

B, Quartz grain from Ajibik quartzite showing numerous lamellae (photomicrograph by Fairbairn, 1942).

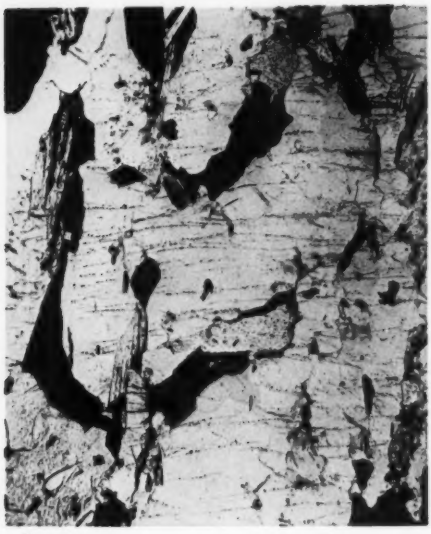


A

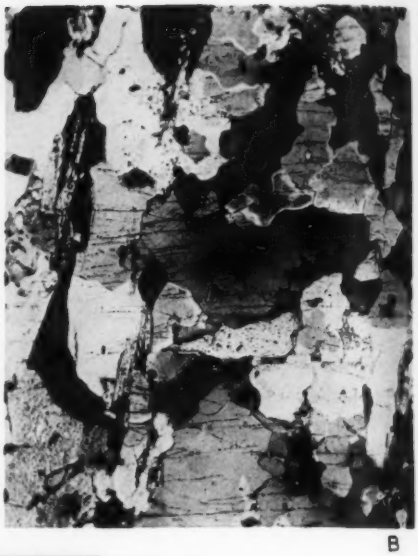


B

Photomicrographs of quartz grains showing liquid inclusions (upper) and numerous lamellae (lower)



A



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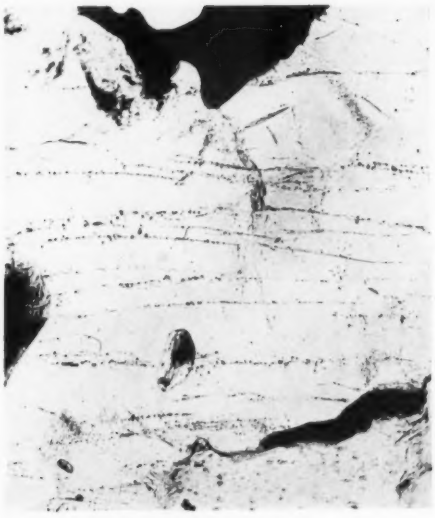


C

Photomicrographs of thin section of gneiss

the host crystal. In many cases this index difference causes the cavities to appear opaque; in others it is obvious that the inclusion is filled with air. This apparently explains the common description in the literature of "rows of vapor, fluid, and opaque inclusions."

Liquid inclusions occasionally consist of two immiscible liquids and a vapor bubble. Gentle heating of this type of inclusion above 31° C. causes one liquid to disappear, indicating that it is carbon dioxide. Cubes of sodium chloride and potassium chloride are occasionally seen in liquid inclusions.

Plate 1, *A*, is a photomicrograph of a plane of secondary liquid inclusions. The plane is perpendicular to the microscope axis. The inclusions in the upper left-hand part of the photograph have boundaries that suggest imperfect negative crystal faces (see p. 334 for discussion of this feature).

ORIGIN OF SECONDARY PLANES OF LIQUID INCLUSIONS

Three different hypotheses have been proposed by various writers for the origin of planes of liquid inclusions. Two of the hypotheses require a secondary origin for the planes, whereas the third requires that the planes be primary. They may be termed (1) fracture, (2) solution, and (3) growth.

The fracture hypothesis was apparently first proposed by Hicks (1884). In describing planes of liquid inclusions in quartz grains of a conglomerate, he noted that they passed from one grain to another of different orientation without deflection. This led him to believe that they were secondary, that is, fractures which were "closed up by a secondary deposition of quartz."

The solution hypothesis is essentially chemical and was first proposed by Judd (1886). He believed that the planes of inclusions were formed by solution entering along "planes of easy solubility." These solution planes, writes Judd (p. 82), ". . . like the cleavage planes and glide planes, have definite relations with the symmetry of the particular system to which the crystal belongs." It follows, then, that planes formed in this manner should have definite crystallographic relations to the host crystal. This, of course, is not the case, as even Judd recognized, for later (p. 90) he writes: ". . . streams of bubbles are seen running through all the grains of quartz, quite irrespective of their orientation."

The third hypothesis for the origin of the planes of inclusions was first advanced by Dale (1923). The exact mechanism is not clear, but it is apparently a growth phenomenon, as he writes (p. 25): ". . . during the consolidation of granite, when the quartz . . . was crystallizing, the fluidal cavities were formed. . . ." Dale also recognized that some planes of inclusions were secondary, but he believed that those in granites were largely primary.

PLATE 2

A, Photomicrograph of thin-section of gneiss showing planes of liquid inclusions as lines or rows of dark specks in quartz grains. ×30.

B, Same as plate 2, *A*, with crossed nicols. The planes of liquid inclusions can be seen to pass from one grain to another of different orientation without deflection at the boundary. ×30.

C, Center portion of plate 2, *A*, at higher magnification. The planes of liquid inclusions are nearly vertical, hence they appear as lines. The undulatory nature of the planes can be seen here. Some individual inclusions can be seen along each plane at this magnification. ×100.

The investigation described in the following pages shows that planes of liquid inclusions in quartz grains of crystalline rocks have a high degree of orientation. The orientation in general is as pronounced as, say, the orientation of mica crystals in a well-foliated rock. This immediately suggests that any theory explaining their origin must include a process for aligning the planes into subparallel orientation over wide areas. The author's field work in the District of Columbia area shows an orientation persistent over an area of 200 square miles which therefore requires a regional deformation. It appears, then, that planes of liquid inclusions are a deformational structure of the same general type as cleavage, jointing, or faulting.

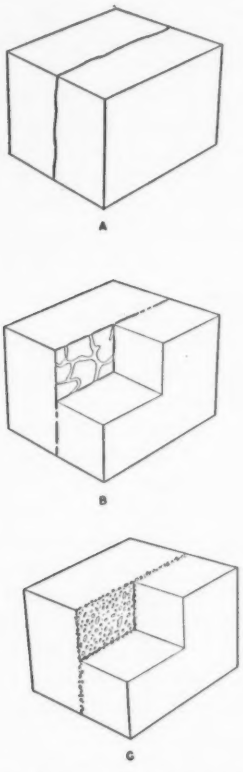


FIG. 1.—Schematic block diagrams illustrating the origin and development of a plane of liquid inclusions. *A*, the first stage in the development of a plane of liquid inclusions is believed to be fracturing of the quartz with subsequent filling of the fracture by intergranular or hydrothermal liquid. *B*, differential solution and deposition of silica have developed individual, thin, sheetlike inclusions. As the inclusions age, *C*, continued solution and deposition change the shape from thin sheetlike to more nearly equant. At this stage crystallographic planes appear as boundaries (see pl. 1, *A*) to the inclusions.

The author's theoretical mechanism for producing a single plane of liquid inclusions is illustrated in figure 1. The first step is fracturing of the quartz grains and filling of the fracture with liquid. Individual inclusions are then formed along the fracture by differential solution and deposition of silica.

The first stage in this final process of solution and deposition is the formation of large, thin, sheetlike inclusions having an extremely irregular outline. This type, when viewed in thin section on the universal stage, almost disappears when turned parallel to the microscope axis. Continued differential solution and deposition of silica gradually change the shape of the individual inclusions from thin sheets to more nearly equant bodies. The influence of crystallographic directions becomes apparent after the process has continued for some time, so that crystal planes appear as boundaries to the inclusions. At this stage the individual inclusions no longer lie in a sharp plane but have grown laterally to some extent, so that the plane has considerable thickness (*C*, fig. 1). Planes of liquid inclusions have been observed in all stages of development from a thin fracture filled with liquid to a broad band of nearly equant inclusions, many of which are bounded by crystal planes.

This suggests a method of estimating the relative ages of two or more sets of planes of liquid inclusions. The method can be illustrated in a mica schist in the

Washington, D.C., area, where an independent check is afforded by the relation of the two sets of liquid inclusions to small drag folds. The older set of planes is made up of nearly equant inclusions, whereas the younger set consists of thin, sheetlike inclusions. The older set of planes is not symmetrically related to the drag folds, and it has been established that these planes were rotated during the drag folding (fig. 77). On the other hand, the younger sheetlike planes are symmetrically related to the drag folds and are believed to be genetically related. The set of sheetlike planes would certainly have been considered younger without the independent evidence introduced by the drag folds.

The difference in age of the two sets of planes can be established by another character of the inclusions. This is illustrated by a thin section of a granite (locality unknown) containing two sets of planes, one of which has inclusions consisting of three phases: water, liquid carbon dioxide, and vapor bubble, whereas the other set consists of inclusions containing only water.

Solution and deposition along a fracture in a crystal can be impelled by mechanical or thermal energy. As differential pressures are certainly present, the so-called "Riecke principle" (1895) may be operative, and mechanical energy may motivate solution and deposition. This principle requires that, in the presence of saturated solution and differential pressure, solution occur at points on a crystal under greatest stress and that precipitation take place at points of least stress. Immediately after fracture, the two sides of the fracture would be in contact over a small proportion of the total area, and these portions of the crystal would be subjected to a greater stress than neighboring areas which are not in contact. Consequently, solution will take place in these highly stressed areas, and deposition will occur in the regions that are not in contact. The net effect of this process will be to increase the area under stress, thereby decreasing the stress per unit area. The area under stress would increase only by decreasing the area not stressed, and the end could give nearly equant liquid inclusions.

A second mechanism for differential solution and deposition would be important if the temperature of the crystal oscillated frequently. Increased temperature would cause silica to be taken into solution in the liquid of the inclusion (if the solubility increases with increasing temperature), and subsequent cooling would cause precipitation on the walls of the inclusion. It seems reasonable to assume that crystal faces would develop with continued solution and deposition, just as a growing crystal assumes euhedral faces.

Theoretically, it also appears reasonable that an irregular inclusion is not at its lowest possible energy level, and, given sufficient time—even without temperature oscillation—solution and deposition would take place until the inclusion has reached this lowest level. This would in all probability be expressed by a liquid inclusion bounded by crystal planes.

The Riecke principle would certainly be operative immediately after the fracturing of the quartz and would be responsible for initiating solution and deposition of silica. Later in the process the other two methods would in all probability be more important in continuing the solution and deposition.

METHOD OF STUDY

The orientation of the planes of liquid inclusions is measured in oriented thin sections in the conventional manner. The trace of each plane of inclusions to be

measured is brought into coincidence with one of the cross-hairs by rotating the inner vertical axis of the universal stage. By rotation on the horizontal axis parallel to this trace, the position at which the plane of inclusions appears as a very fine line, and hence parallel to the microscope axis, can be determined with considerable accuracy.

FIG. 2*a*.—Locality 19 (see fig. 25). 98 poles to planes of liquid inclusions measured by carefully aligning only those portions of each plane that are essentially planar. Contours 25–20–15–10–5–2–0 per cent.

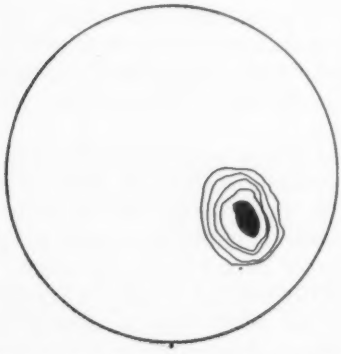


FIG. 2*b*.—Locality 19. 100 poles to planes of liquid inclusions from the same thin section as *2a*. Measured by approximating the strike and dip of each plane. There is a slight loss in detail using this method which is similar to that used in measuring a rough joint or foliation plane in the field. Contours 25–20–15–10–5–0 per cent.

Identification and measurement of planes of liquid inclusions can be greatly expedited by preparing thin sections four or five times the normal thickness. Planes of large, equant liquid inclusions cannot be measured at all in a section of normal thickness. They appear as a row of inclusions which do not show a planar direction when rotated about a horizontal axis on the universal stage, thus accounting for the common description in the literature of "rows of liquid, gaseous, and opaque inclusions."

Planes of liquid inclusions are seldom uniform in the geometrical sense. The word "plane" is used here in the same sense as the geologist uses "fault plane" or "joint plane." The planes undulate considerably, and, when aligning the "strike" of a plane parallel to a cross-hair of the microscope, it is sometimes necessary merely to approximate the average "strike." When this question came up early in the investigation, several fabric diagrams of the planes of liquid inclusions were prepared from the same thin section, using slightly different techniques in aligning the planes of inclusions. First, a diagram was prepared by plotting only those portions of the planes that were actually planar. Another diagram from the same section, made by approximating the general strike of the planes in the microscope field, gave a similar diagram. Figure 2 illustrates the magnitude of the difference when the two methods are used. The differences illustrated indicate that some detail is lost by the second method of measurement; however, for the present reconnaissance work the second method is entirely adequate.

A more important source of error in this type of measurement is introduced through improper orientation of the thin sections with respect to the planes of in-

clusions. Figure 3 illustrates this error. In making the traverse AA', nine planes of inclusions would be recorded, representing the vertical set of planes, with no recordings of the equally numerous horizontal planes. If the traverse were made along BB', seven planes of each orientation would be recorded, this being the correct relation, provided that both sets of planes were perpendicular to the surface measured. If one set of the planes were nearly vertical while the other dipped steeply, neither traverse method would be correct (CC'–DD').

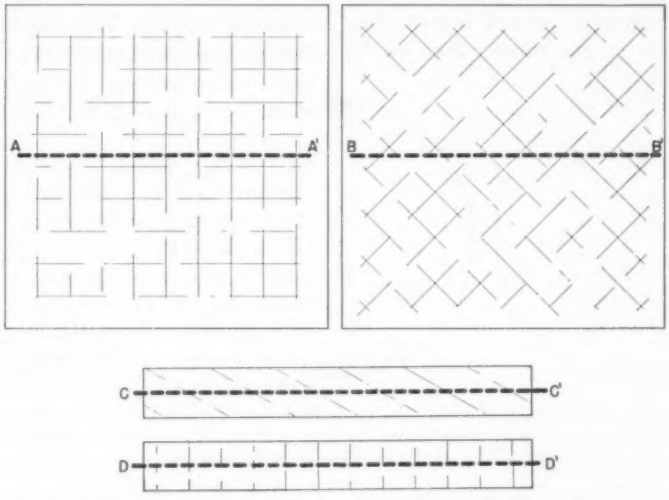


FIG. 3.—Schematic diagram illustrating some problems in traverse methods encountered when making statistical analysis of two or more sets of planes of liquid inclusions.

A mathematical expression can be derived for corrections where the thin section is not approximately perpendicular to the planes of inclusions. However, it was found to be much less time-consuming merely to cut sections perpendicular to each set of planes as determined in an initial section. Billings and Sharp (1937) have discussed similar problems encountered in making statistical studies of micas.

ORIENTATION IN INDIVIDUAL QUARTZ GRAINS

The most striking feature of these secondary planes of liquid inclusions in single quartz grains is the lack of crystallographic control of their orientation. This can readily be seen by crossing nicols and observing the planes extending across many quartz grains of different orientation with no deflection at the grain boundaries. A single plane within a quartz grain is usually nearly planar. However, the planes undulate considerably within a single grain, indicating, again, the lack of influence of crystallographic planes in the quartz.

Plate 2, *A* and *B*, illustrates the general character of the planes of liquid inclusions. The dark lines or rows of specks are planes of liquid inclusions. The planes are essentially vertical and appear to be continuous rather than made up of separate liquid inclusions. Plate 2, *C*, is a photomicrograph of the large grain in the center of plate 2, *A*. The undulating

nature of the planes of liquid inclusions within a single grain can be seen at this magnification.

Figure 4 is a plot of the poles to planes of liquid inclusions in large single quartz grains. Where the planes were appreciably undulatory, several measurements were made at intervals along the plane, and the resulting plot of the poles connected by a line. These three diagrams were made from three different quartz grains in the same thin section. The lack of crystallographic control is well illustrated here, as the three *c*-axes of the grains have considerably different orientation, whereas the poles of the planes of liquid inclusions all have essentially the same orientation. This is further illustrated in figure 5, which was prepared by traversing the same thin section and plotting the *c*-axes of quartz grains together with the poles to planes of liquid inclusions. The high degree of preferred orientation of the planes of liquid inclusions with respect to the almost random orientation of the *c*-axes of the quartz grains further indicates the lack of control of the planes of liquid inclusions by crystallographic directions.

The planes of liquid inclusions in one quartz vein from the Washington, D.C., area are so unusual and informative that a detailed description and discussion is worth while. The usual shear planes of inclusions (see p. 347) locally deviate toward the direction of tensional rupture. A single plane of inclusions curves into short en echelon breaks, with the trend of the tensional breaks along the same direction as the original shear break (fig. 6). This resembles, on a small scale, shear faults (Newhouse, 1942) locally deviating toward the direction of tensional rupture. This has taken place *within* single quartz grains, indicating, perhaps, that the quartz behaved more like an isotropic material than like an anisotropic crystalline substance.

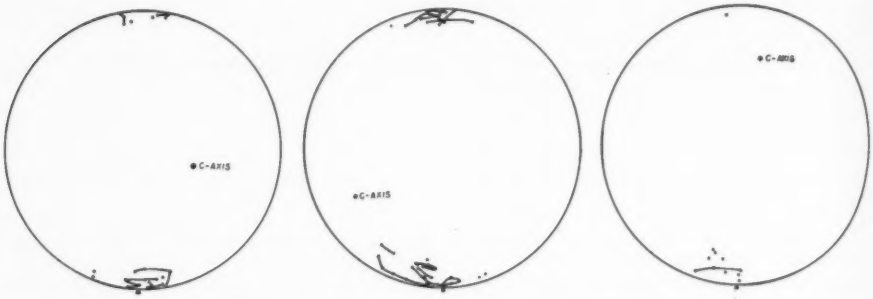


FIG. 4.—Orientation of planes of liquid inclusions in single quartz grains. Lines connecting poles illustrate the undulatory character of many of the planes. All three quartz grains are from the same thin section. The lack of crystallographic control of the orientation of the planes is apparent.

Planes of liquid inclusions vary greatly in their abundance in quartz grains. Thin sections of a specimen from locality 33 (see pl. 2, *A*, and fig. 27), for example, have planes spaced approximately at 0.06 mm., so that a traverse of 3 mm. perpendicular to the planes gave fifty measurements for a fabric diagram (fig. 59). This is one of the most abundant occurrences encountered. On the other hand, thin sections from locality 12 have only four or five planes that could be identified in completely traversing two large thin sections.

In general, planes of liquid inclusions appear to be most abundant in rocks containing a high percentage of quartz (i.e., quartzites). In some specimens showing a dearth of planes of liquid inclusions the foliation has nearly the same orientation as the planes elsewhere. It is suspected that movement which might have produced fractures that subsequently would become planes of inclusions was taken up by the mica, which was responsible for the foliation.

The average thin section from the Washington, D.C., area contains enough planes to prepare a representative fabric diagram on traversing the section four or five times.

In summary, the planes of liquid inclusions in individual quartz crystals show little or no relation to crystallographic directions in the quartz. In many respects the planes appear to have formed by rupture, with the directions of the rupture being controlled directly by the deforming stress uninfluenced by crystal structural planes within the quartz.

ORIENTATION IN SINGLE HAND SPECIMENS

One of the most striking features of fabric diagrams of planes of liquid inclusions is their high degree of orientation. Maxima up to 45 per cent in a 1 per cent area are not unusual, and 20 per cent maxima are very common. The diagrams look very much like mica diagrams from a well-foliated schist.

In the Washington, D.C., area, diagrams of planes of liquid inclusions commonly have only one strong maximum, as illustrated in figures 9 and 10. The planes of inclusions, then, usually occur in a single orientation, with only slight deviation from that orientation. Figures 11 and 12 are characterized by "split" maxima. This type has been found repeatedly in the Washington area (see p. 336). The genetic significance of these split maxima is not known. They may represent some slight crystallographic influence, or possibly they represent a slight change in the over-all stress relations at the time of their formation. If the latter, they should be characteristic of the area. It will be interesting to see what further work of this nature discloses elsewhere.

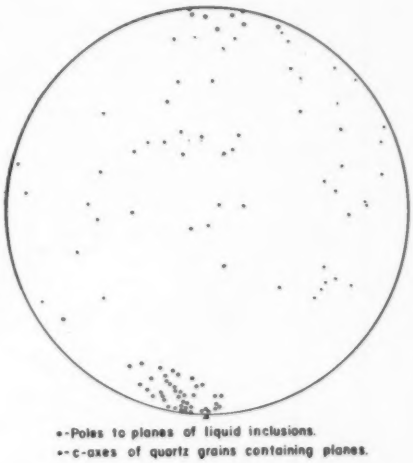


FIG. 5.—Diagram illustrating the relation between poles to planes of liquid inclusions and *c* axes of quartz. Prepared from the same thin section as fig. 4. The lack of crystal structure control of the orientation of the planes of liquid inclusions is illustrated here.

In contrast to the single strong maximum found in the foliated rocks of the area, two or more distinct maxima have been found in some quartz veins (figs. 13 and 14). The planes of inclusions responsible for the single maximum in the foliated rocks do not continue into the quartz veins. This might indicate that the quartz veins were introduced later than the fracturing that subsequently be-

came planes of liquid inclusions (see p. 342).

Descriptions of planes of liquid inclusions from other areas frequently report "two sets of planes nearly at right angles." To check these observations, ten thin sections with a wide geographic distribution were selected and the orientation of planes of inclusions measured. The results are illustrated in figures 15–24, inclusive. As only one section was available in all cases, the diagrams are incomplete, in that other concentrations of poles to planes of inclusions may occur near the center of the diagrams (planar structures cannot be measured when nearly parallel to the section). Five of these ten sections show two sets of planes "nearly at right angles" (figs. 15–18, 22). Three of the diagrams (figs. 20, 21, 23) have maxima similar to those found in the Washington, D.C., area.

Many fabric diagrams of the poles to planes of liquid inclusions may have incomplete girdles. One specimen (fig. 24) has a complete girdle. It appears that the girdles occur about the fold axis, indicating that the planes are intersecting in the *b* fabric axis.

ORIENTATION IN SINGLE OUTCROPS

In a single outcrop, planes of liquid inclusions do not deviate to a measurable extent in hand specimens collected across the outcrop. Figure 7 illustrates this uniform orientation. (The slight difference in orientation of the maxima is within the experimental errors introduced during collecting, preparing the section, etc.) The planes cut across compositional boundaries with no deflection. For example, in figure 7 the planes have exactly the same orientation in the hornblende gneiss, the quartz lenses, and the

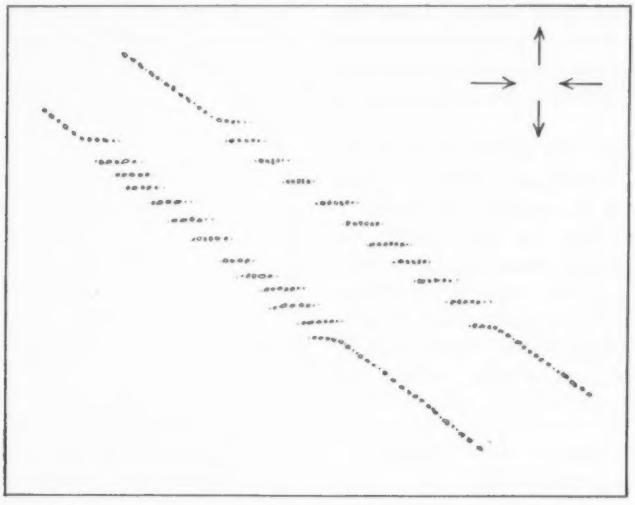


FIG. 6.—Sketch of the planes of liquid inclusions in a single quartz grain. The sketch is somewhat idealized. The planes of inclusions are perpendicular to the plane of the paper, and the rows of irregular circles and dots represent the trace of the planes on the surface sketched. The short en echelon planes are believed to be tensional breaks, as indicated by the arrows. The trend of the en echelon planes is believed to be the shear direction of the deforming stress pattern.

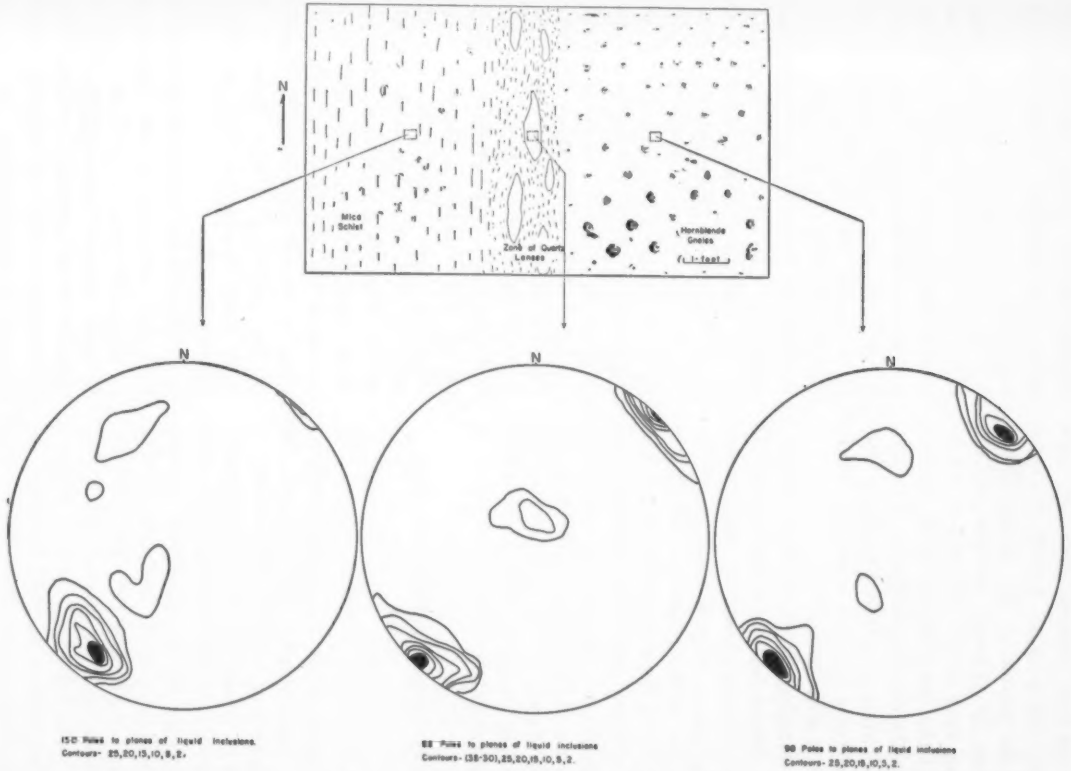


FIG. 7.—Sketch of an outcrop showing the orientation of planes of liquid inclusions in the different rock types

biotite schist. However, the number of planes per unit volume may vary considerably (see p. 339). A 1-cm. traverse across the planes in the quartz lenses may cross fifty planes, whereas only ten or fifteen may be encountered in a traverse of the same length in the biotite schist or the hornblende gneiss. This uniform orientation within the single outcrop has been found at numerous localities in the area studied.

In contrast with this uniform orientation in various foliated rocks, the quartz veins of the area present an entirely different problem. Figure 8 illustrates the orientation of the planes in two quartz veins and the enclosing mica schist. The planes of liquid inclusions in these veins show no obvious symmetrical relation to the planes in the schist. This suggests that the planes in the quartz veins are late and postdate the planes of inclusions in the surrounding rocks.

ORIENTATION IN WASHINGTON, D.C., AREA

AREAL GEOLOGY

The rocks of the area investigated are largely metamorphic and igneous, the only exception being a thin veneer of flat-lying, coastal-plain, sedimentary rocks in the southeastern portion. The metamorphic rocks are highly foliated with well-developed lineation. Lithologically the rocks are extremely diverse. This diversity is so pronounced that it seems certain that, if several different geological parties mapped the area, the resulting maps would show little, if any, uniformity of areal distribution of formational units.

Most of the southern half of the metamorphic belt was mapped by Williams and Keith in 1893–99. The rocks are classified as Archean and the following types described: granite, granite gneiss, diorite, diorite gneiss, gabbro, metagabbro, metadiorite, serpentine, and soapstone.

Over 90 per cent of the area studied (fig. 27) is composed of rock types that were called "granite gneiss" or "diorite gneiss" by Williams and Keith (1901). These rocks are divided into two areas by a biotite granite which occurs in a narrow belt about 1 mile wide, running north-south through the entire region studied.

The granite and diorite gneisses are characterized by the almost universal presence of rock inclusions. The inclusions are usually lenticular, with their planar direction parallel to the foliation. They range in size from a few millimeters to a few meters in largest dimension. The smaller inclusions are usually quartz, although micaceous, chloritic, and pyritiferous inclusions are seen in places. The inclusions commonly have a bladelike habit, with the long axis parallel to the *b* lineation of the enclosing gneiss. The large inclusions commonly show structures that appear to be remnants of bedding or schistosity, which is usually parallel to the foliation of the enclosing gneiss. However, in some cases divergence from parallelism is pronounced.

These large inclusions would doubtless be called xenoliths by one disposed to consider this type of rock as intrusive igneous, whereas an advocate of granitization would probably interpret them as remnants of the original rock that had escaped the granitization.

The biotite granite is characterized by the absence of inclusions and by its uniformly light color. The biotite occurs in large wavy flakes that give the rock a pronounced speckled appearance. Foliation is not so pronounced as in the sur-

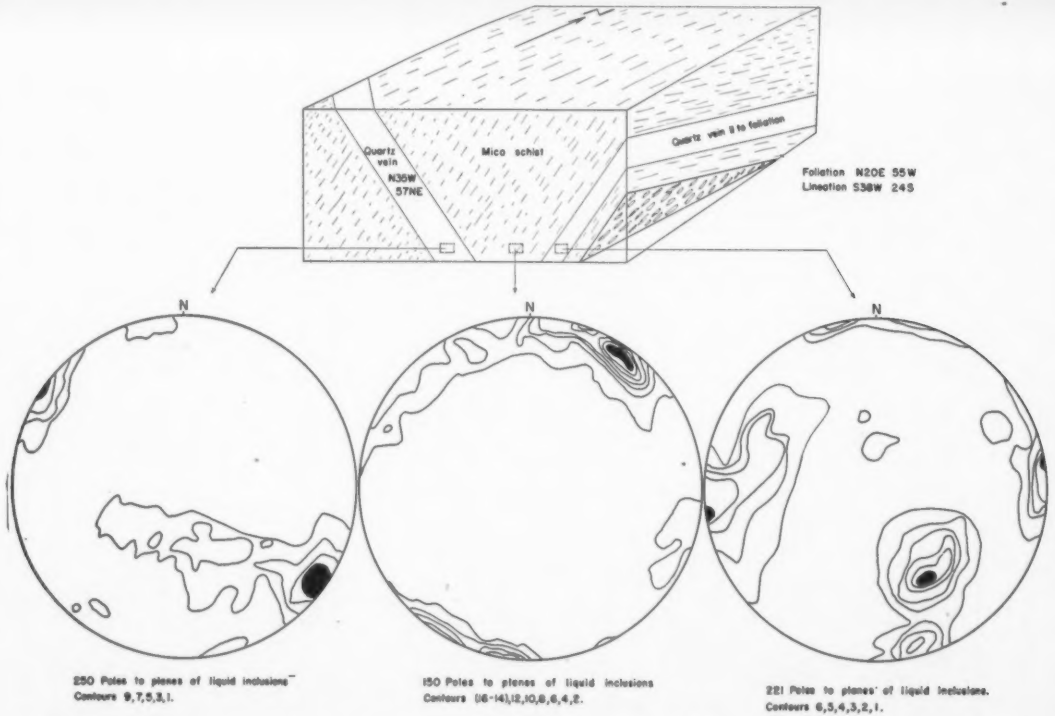


FIG. 8.—Sketch of outcrop relations of mica schist and quartz veins, with fabric diagrams showing the orientation of planes of liquid inclusions. The quartz vein parallel to the foliation is approximately 1 foot thick where exposed, and the cross-cutting vein is approximately 2 feet in thickness.

rounding gneisses, except near the contacts and in the northern portion of the area. Lineation is poorly developed except in the northern part of the area.

STRUCTURAL FEATURES

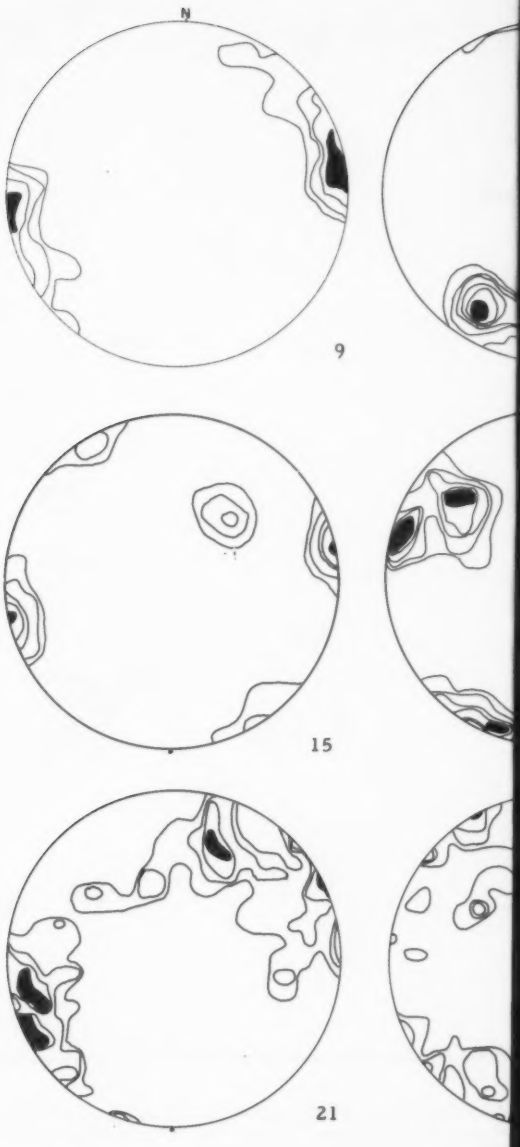
Perhaps the most prominent structural feature of the region is the foliation (fig. 28). With the exception of a few minor areas, all the rock types encountered are well foliated. Nonfoliated granite was found in only two small areas. The foliation is so pronounced that one is inclined to classify all the rocks in the area as metamorphic rather than intrusive igneous. This tendency is enhanced by the scarcity of cross-cutting contacts. Although some contacts are discordant, in general they are parallel to the foliation, schistosity, or banding. The contacts are also parallel to the planar direction of the ubiquitous inclusions in the granite and diorite gneisses. Some of the more schistose horizons in the granite gneiss show small, close, contorted folds. The axial line of these folds is parallel to the regional lineation, and, in general, the axial planes appear parallel to the regional foliation. The "bedding" on the limbs of these small folds is also parallel to the regional foliation.

Locally the contact between, say, the granite gneiss and a schistose horizon in the granite gneiss may have discordant relations measured in feet. Embayments occur in the schist, and in some places blocks of it are apparently surrounded by the granite gneiss. Petrographic examination of the relations suggests a replacement origin for this type of contact, as fragments of the schist can be seen in thin section in all stages of digestion, with the smallest fragments showing unmistakable parallelism with the surrounding schist. If this were an intrusive contact, one would expect to find some fragments of the schist rotated somewhat by the "intruding" granite gneiss.

Foliation.—The most common type of foliation is a result of the alignment of mica flakes into subparallel orientation. The foliation is locally intensified by segregation of mica into layers alternating with quartzose layers. In some places banding due to alternating layers of amphibole and quartz-feldspar gives a pronounced foliation. In others the foliation is a result of alternate layering of quartz-feldspar and chlorite or of quartz and epidote. Foliation in the biotite granite is made conspicuous by the large, dark-colored mica flakes, but on close examination it is seen that it is in part due to segregation into bands of the quartz and feldspar. Thus, although the rock has the mineral composition of a granite, texturally it is a gneiss.

Lineation.—Five types of lineation have been observed in the Washington, D.C., area (fig. 29). Four of these are believed to be parallel to *b* (fold axis) and one perpendicular to *b* in the plane of foliation. The evidence that the four types are parallel to *b* is given by their parallelism with the axial lines of all the minor folds in the area (see above). The relation of these small folds to any major folds that may exist is not known. It is possible that the fold axes of the small folds may be either parallel or perpendicular to the regional fold axes. If they are parallel, the above-mentioned four types of lineation would be parallel to *b* of both the small folds and the regional folds. However, if the fold axes of the small folds are perpendicular to large-scale regional folds, then the four types of lineation would be in *b* of the small folds and *a* of the regional folds.

The four fold-axis types are: (1) Mica flakes in many of the rocks are so oriented that the poles to the cleavage have a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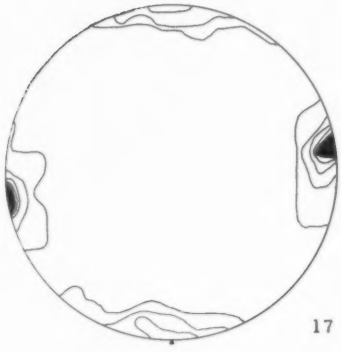

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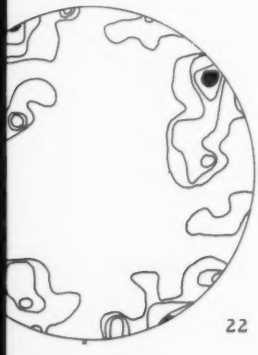

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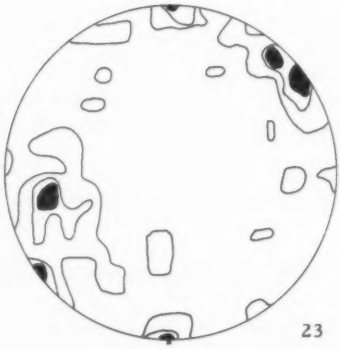

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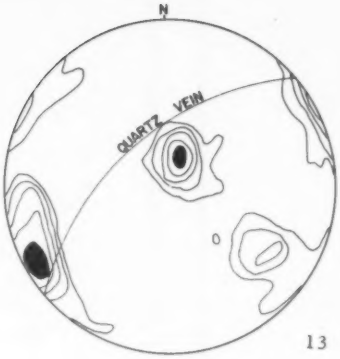

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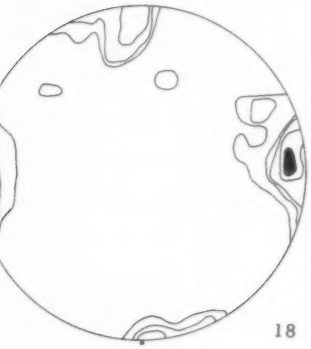

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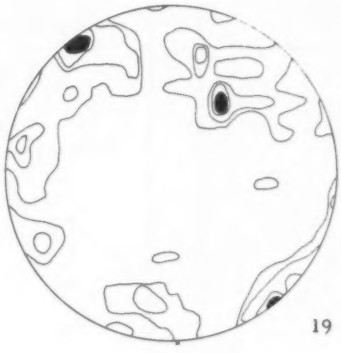

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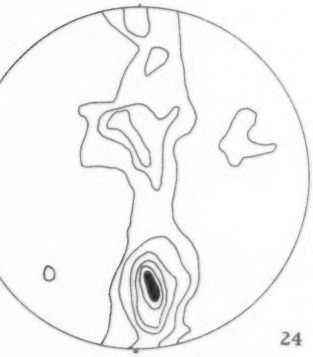

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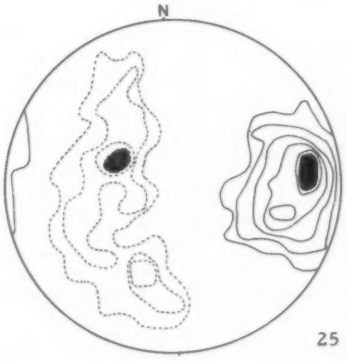

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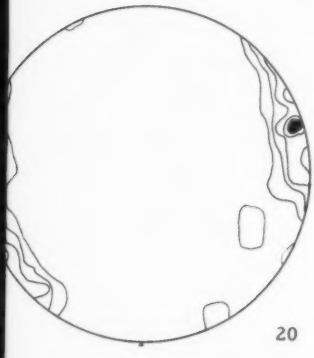

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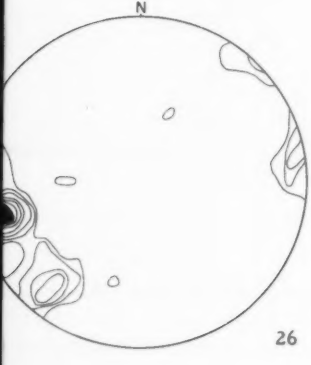

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Figure Number	Locality Number or Location	N L
9	28	
10	23	
11	32	
12	31	
13	17	
14	18	
15	Grand Canyon	
16	Christiana, Norway	
17	Schlesien, Germany	
18	Wiborg, Finland	
19	Cape Ann, Mass.	
20	Zinnwald, Germany	
21	Altenberg, Germany	
22	Hohwald, Germany	
23	Markirch, Ober Elsass	
24	1 mi. S. of Highland, Md.	

Fig. 25.--Compilation of field mea
151 lineation measurements. Contou
210 poles to foliation. Contours (16-

Fig. 26.--Compilation of the orien
to planes of liquid inclusions. Conto

cation	Number of Poles to Planes of Liquid Inclusions	Contours in Per Cent	Rock Type
	50	(20-16)-10-5-2-0	
	50	24-20-16-12-8-4-0	
	50	22-16-10-4-2-0	
	50	22-16-10-4-2-0	
	175	15-11-7-3-1-0	Quartz vein
	221	(6-5)-4-3-2-1-0	Quartz vein
	168	20-15-10-5-2-0	Archean schist
	50	10-8-6-4-2-0	Granite
	54	20-15-10-5-2-0	Granite
	50	(18-14)-12-8-4-2-0	Rappakiwi granite
	50	8-6-4-2-0	Granite
	50	20-15-10-5-2-0	Greisen
	56	8-6-4-2-0	Greisen
	55	8-6-4-2-0	Granite
ss	50	(8-6)-4-2-0	Granite
Md.	154	(14-11)-9-7-5-3-1-0	Quartzite

field measurements of the orientation of foliation and lineation.
. Contours 8-6-4-2-0 per cent.
ours (16-15)-12-9-6-3-1-0 per cent.

the orientation of maxima from 30 statistical diagrams of poles
s. Contours 23-20-17-13-10-7-3-0 per cent.

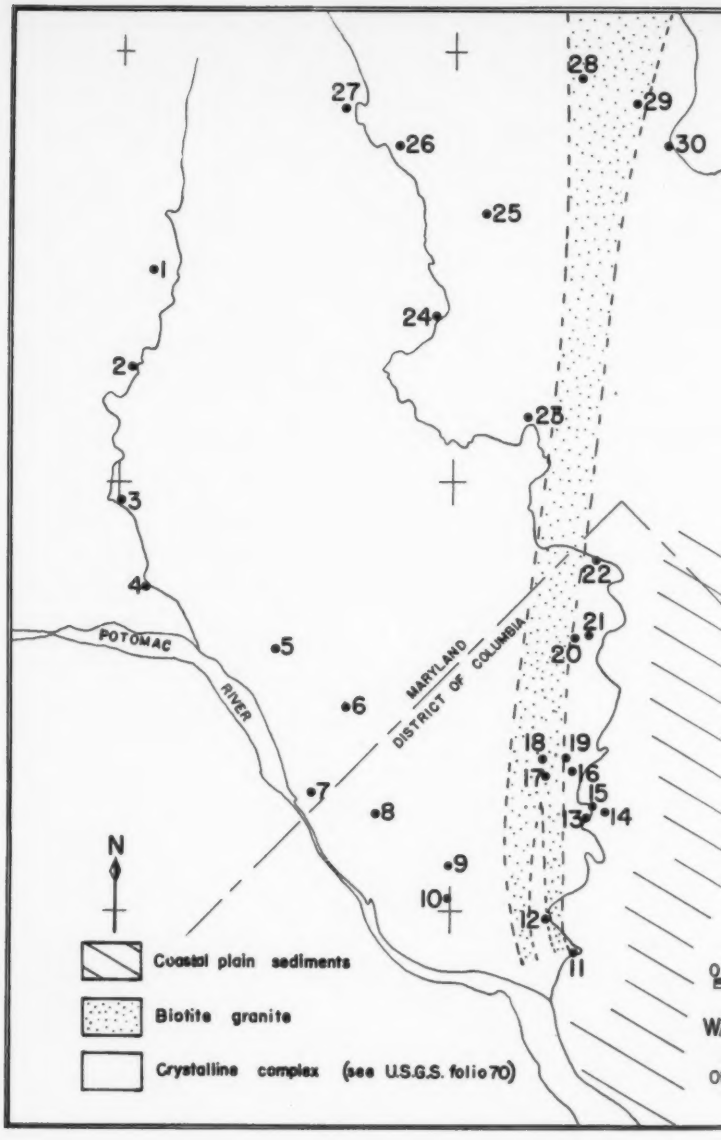
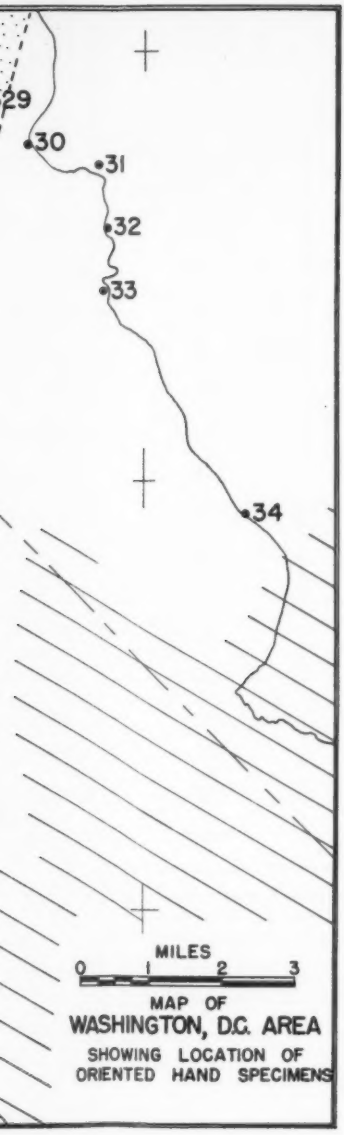


Fig. 27.--Map of the Washington, D. C., area showing the loca
which oriented hand specimens were collected, and the extent o
mass. Diagrams prepared from oriented specimens from this
identified with the above localities.



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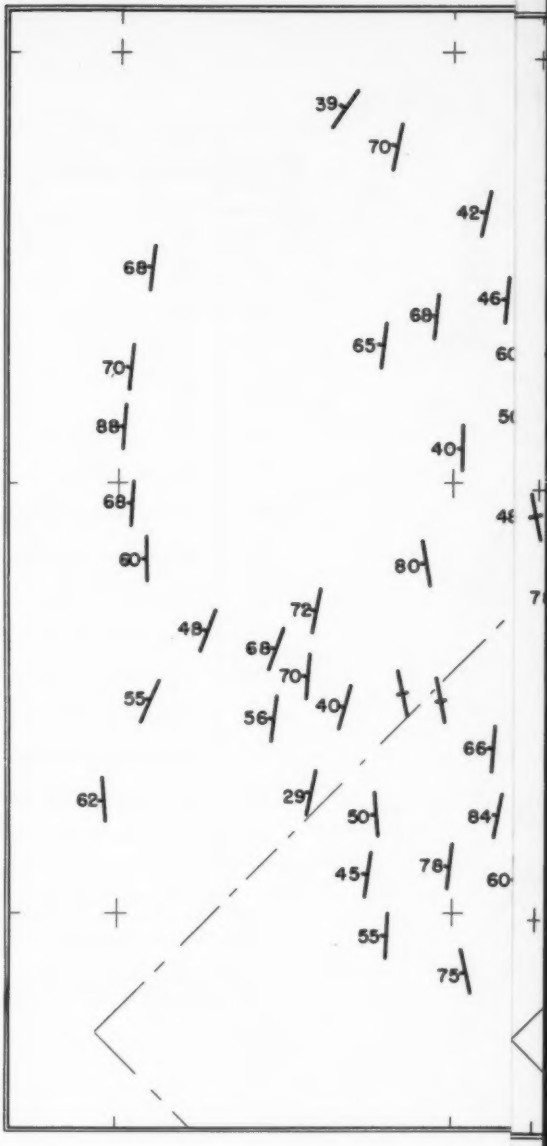
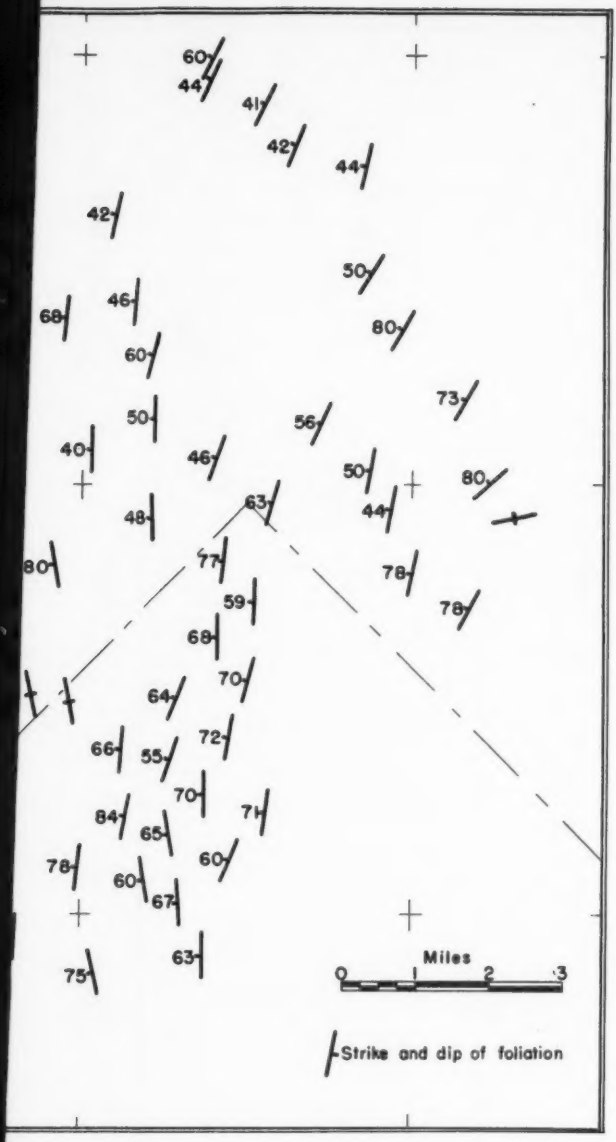


Fig. 28.--Map of the Washington, D. C
foliation.



hington, D. C., area showing the orientation of the

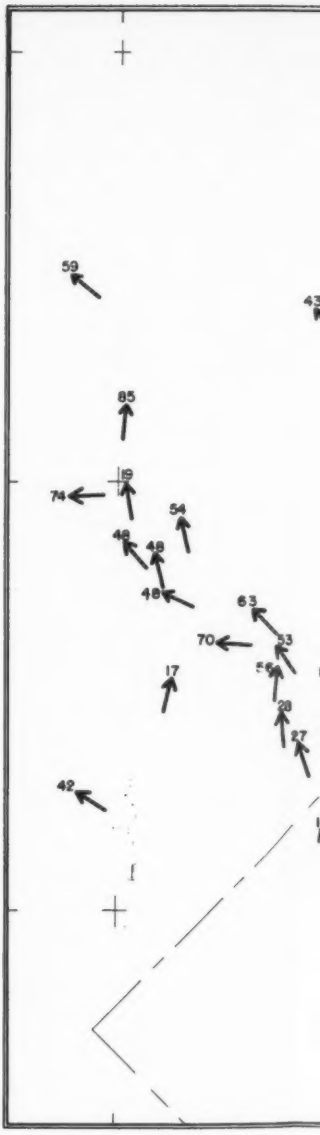
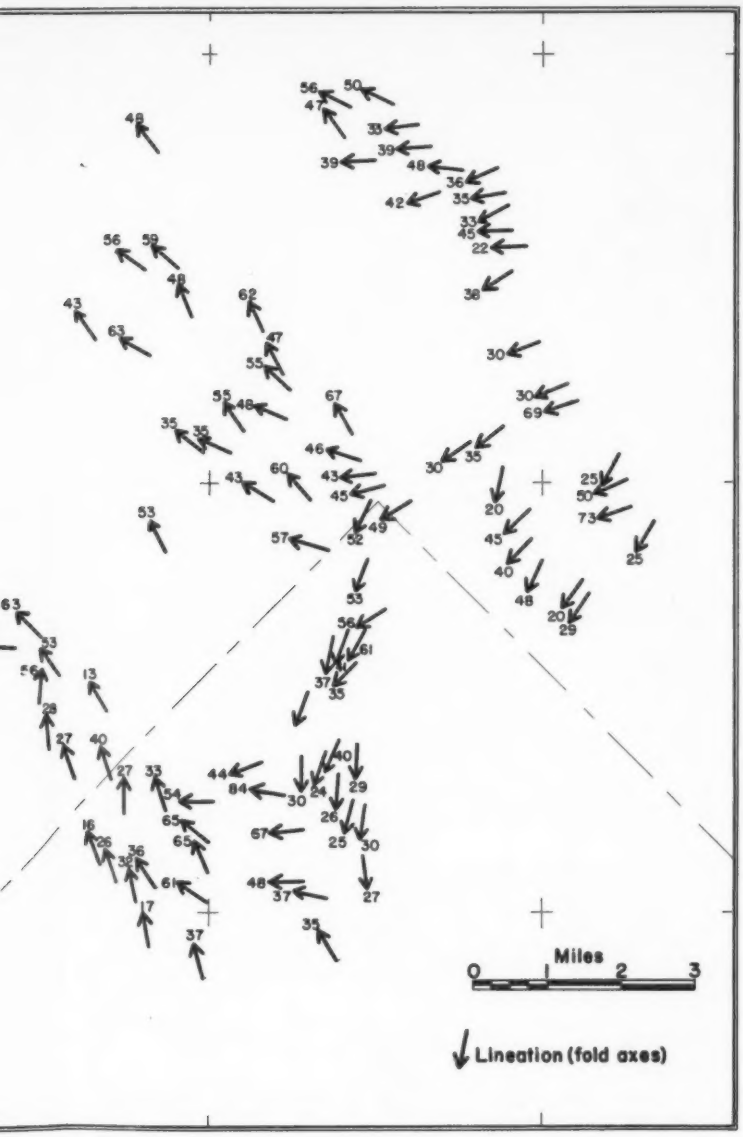


Fig. 29.--Map of the
lineation. These linea
axis type of element (p



Map of the Washington, D. C., area showing the orientation of the
hese linear elements lie in the foliation planes and are the fold
element (parallel to axial lines of folds).

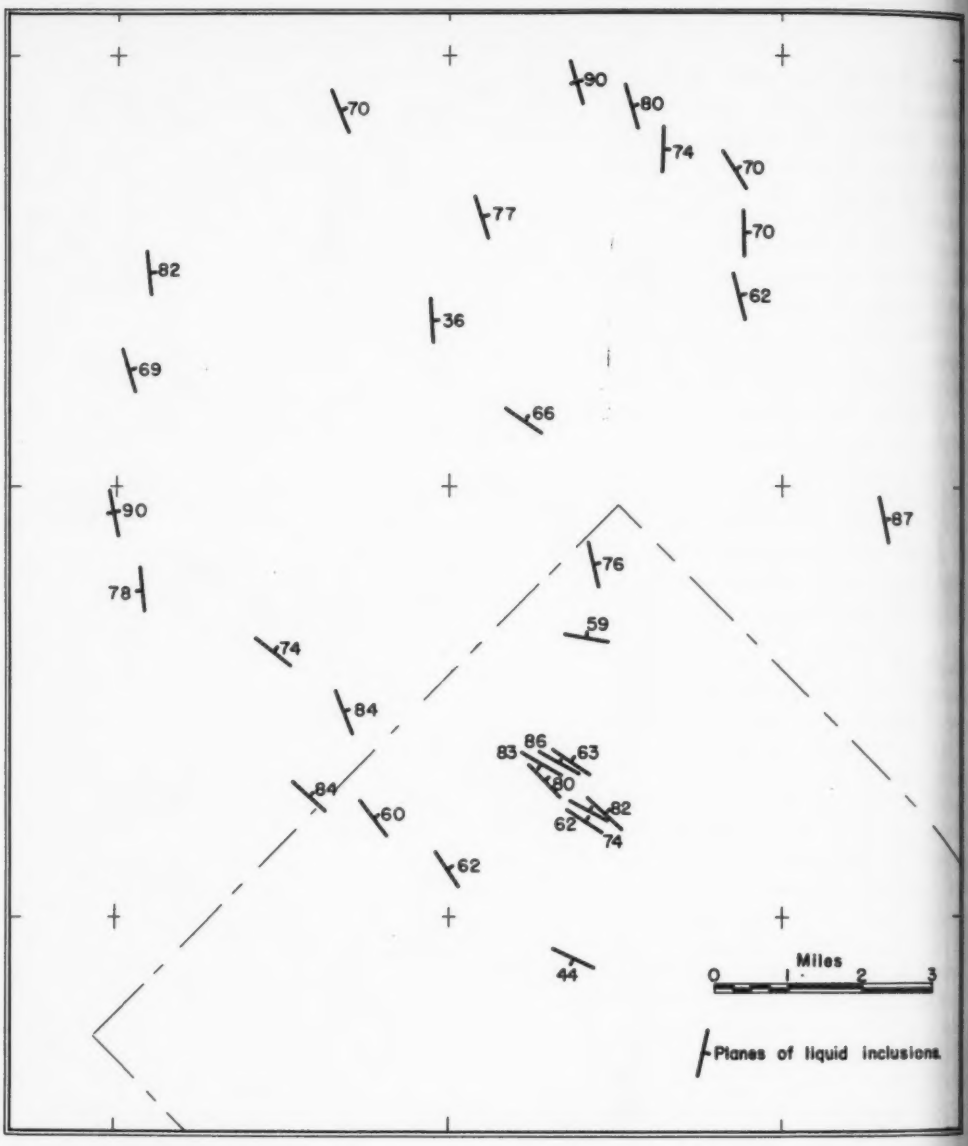
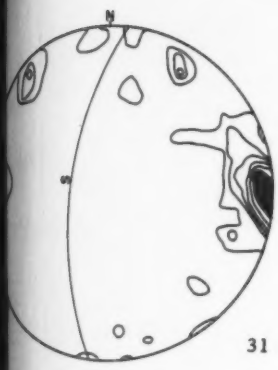


Fig. 30.--Map of the Washington, D. C., area illustrating the orientation of planes of liquid inclusions. Each value shown is approximately the maximum of a statistical fabric diagram. The diagrams from which these values were taken may be found by noting the locality on the index map (fig. 27) and looking up the diagram for that locality. See also figure 26.



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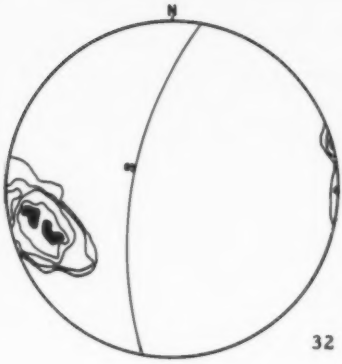

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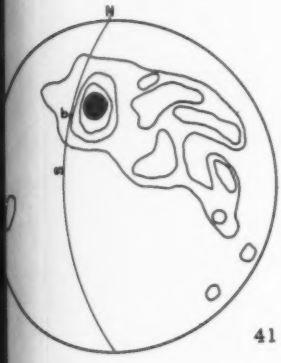

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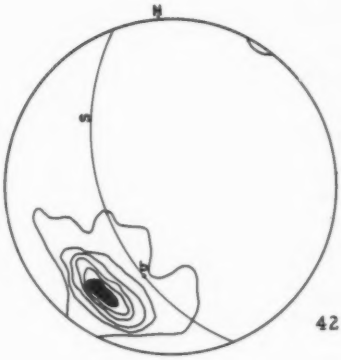

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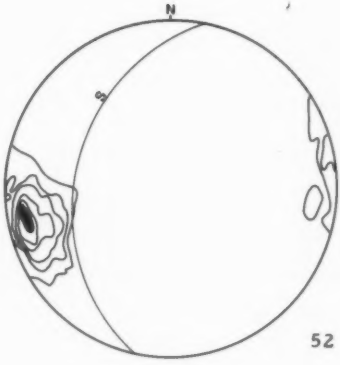

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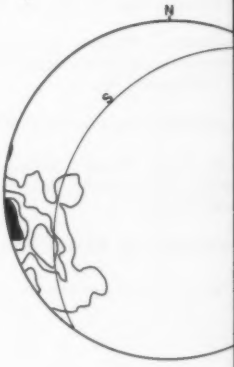

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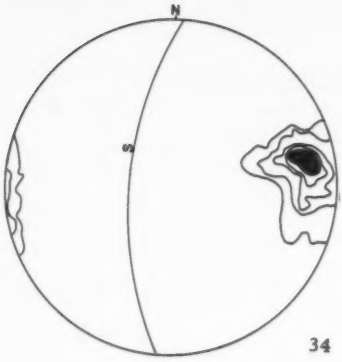

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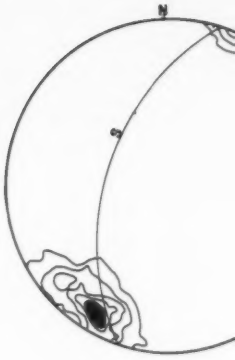

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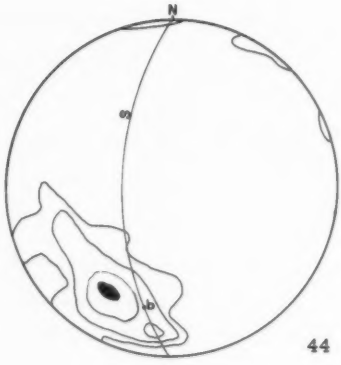

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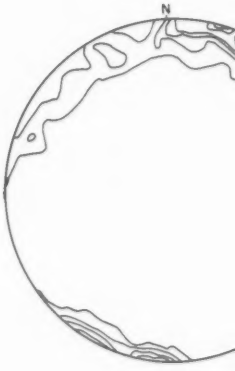

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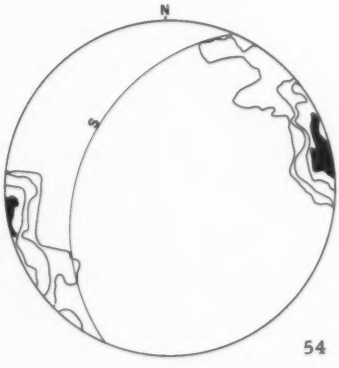

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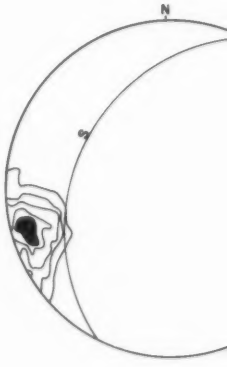

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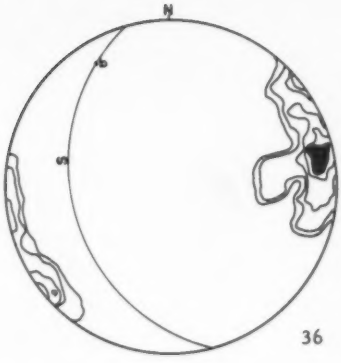

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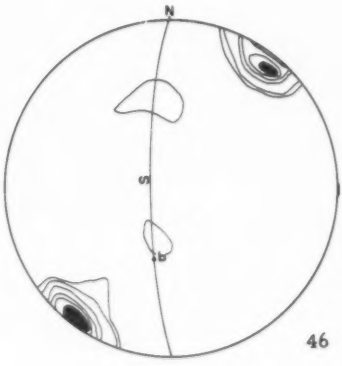

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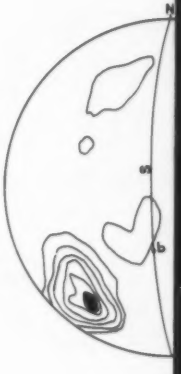

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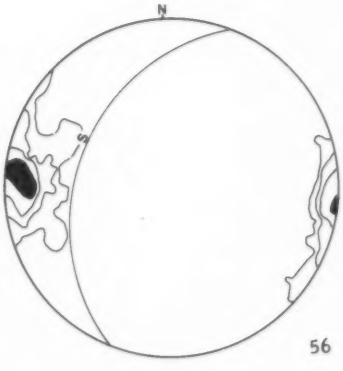

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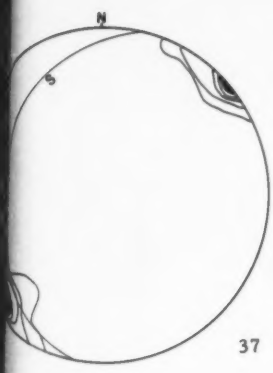

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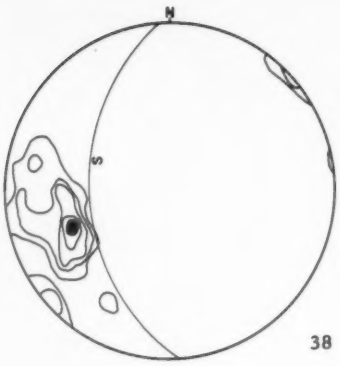

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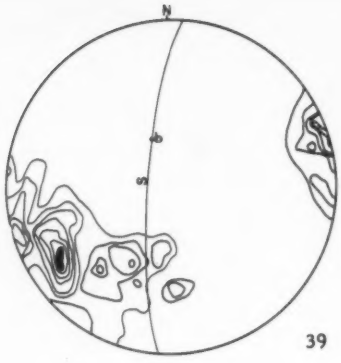

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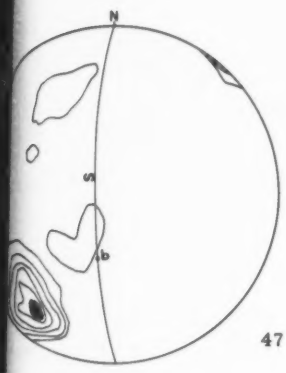

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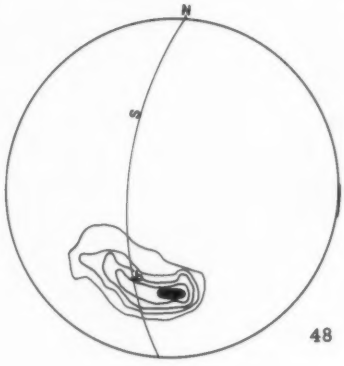

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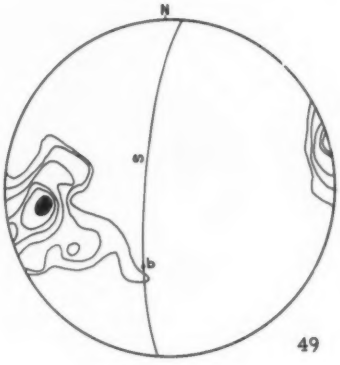

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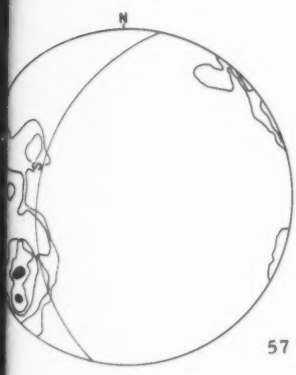

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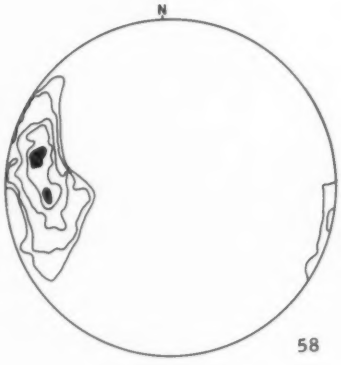

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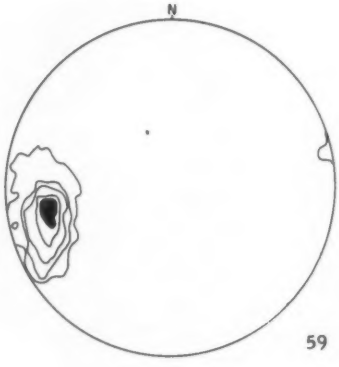

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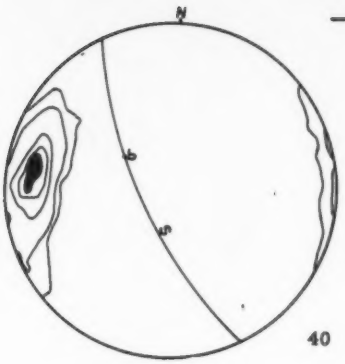

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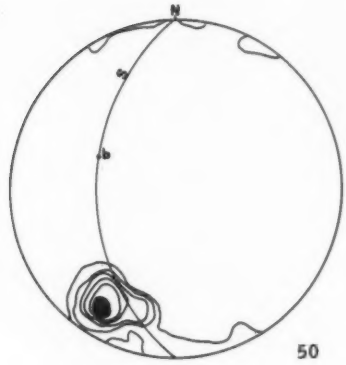

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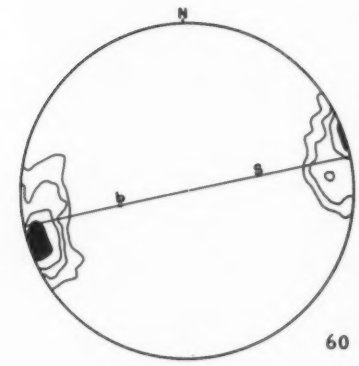

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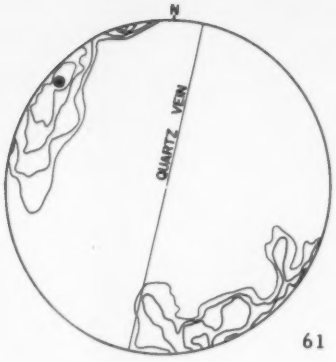
Figure Number	Locality Number	Number of Poles to Planes of Liquid Inclusions	Contours in Per Cent
31	1	36	16-14-11-8-6-3-0
32	2	33	30-21-12-6-3-0
33	3	50	18-14-10-6-2-0
34	4	40	(30-26)-20-14-8-2-0
35	5	50	32-24-16-8-2-0
36	6	50	22-16-10-4-2-0
37	7	50	28-22-16-10-4-0
38	8	50	20-16-12-8-4-0
39	9	50	14-12-10-8-6-4-2-0
40	10	150	20-15-10-4-2-0
41	11	86	(10-8)-6-4-2-0
42	13	140	25-20-15-10-5-1-0
43	14	126	8-6-4-2-1-0
44	15	146	15-10-5-2-0
45	18	150	(16-14)-12-10-8-6-4-2-0
46	19	98	25-20-10-5-2-0
47	19	150	25-20-10-5-2-0
48	21	50	20-16-12-8-4-0
49	22	50	20-16-12-8-4-0
50	23	60	24-20-18-12-8-4-0
51	24	50	20-14-8-4-2-0
52	25	50	32-26-20-14-8-2-0
53	27	50	20-16-12-8-2-0
54	28	56	20-16-10-5-2-0
55	29	50	24-18-12-6-2-0
56	30	50	30-22-12-6-2-0
57	31	50	22-16-10-4-2-0
58	32	50	22-16-10-4-2-0
59	33	50	18-14-10-6-2-0
60	34	50	30-22-14-6-2-0



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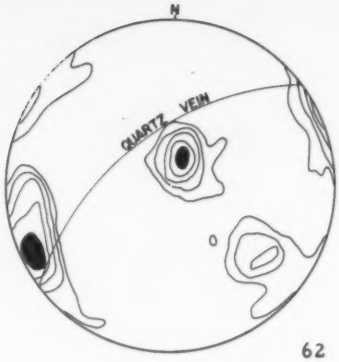

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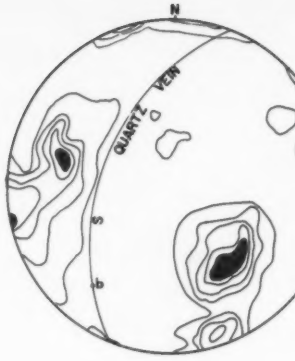

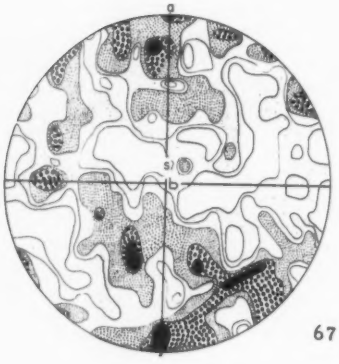

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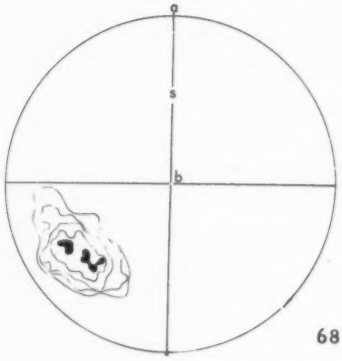

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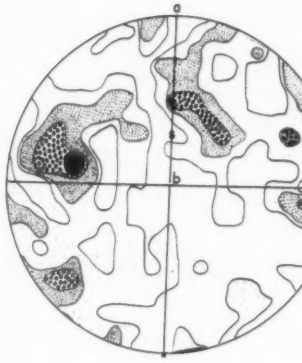

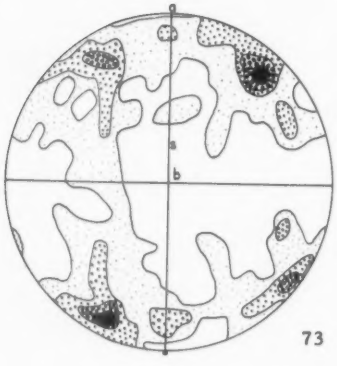

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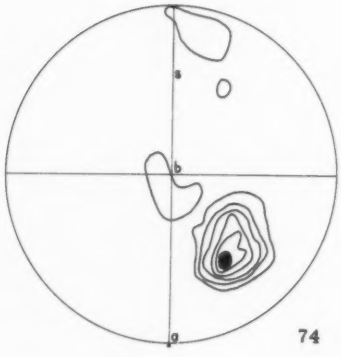

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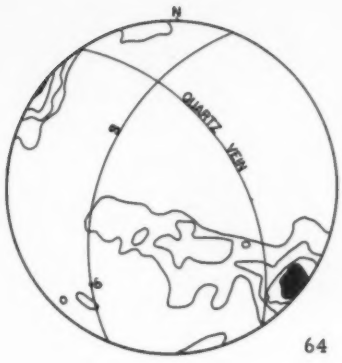

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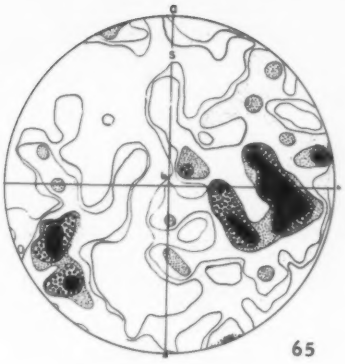

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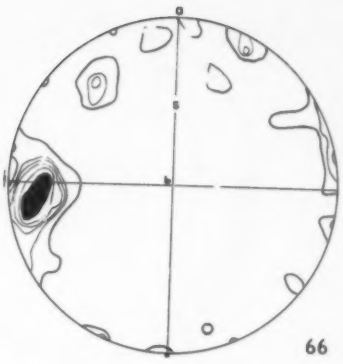

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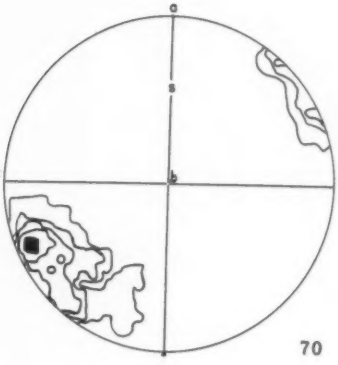

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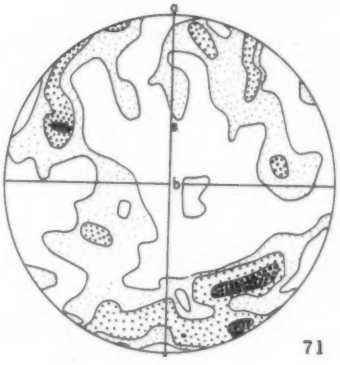

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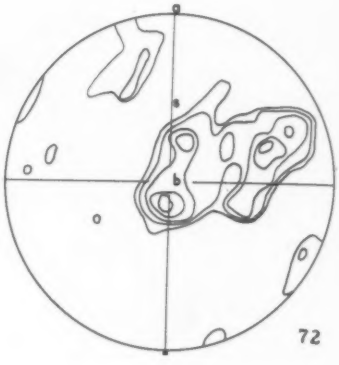

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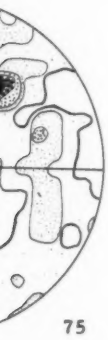

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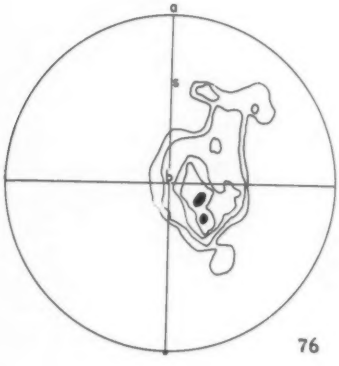

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Figure Number	Locality Number	Number of Poles to Planes of Liquid Inclusions	Contours in Per Cent	Remarks
61	8	53	20-16-12-8-4-0	Quartz vein
62	17	175	15-11-7-3-1-0	Quartz vein
63	18	221	(6-5)-4-3-2-1-0	Quartz vein
64	18	352	(10-8)-6-4-2-0	Quartz vein
65	1		3-2.5-2-1.5-1-0	245 C_v quartz. Planes inclusions from same shown in figure 66.
66	1	36	16-14-11-8-6-3-0	Quartz orientation show
67	2		3-2.5-2-1.5-1-0	300 C_v quartz. Planes inclusions from same shown in figure 68.
68	2	33	30-21-12-6-3-0	Quartz orientation show
69	3		4-3-2-1-0	213 C_v quartz. Planes inclusions from same shown in figure 70.
70	3	50	18-14-10-6-2-0	Quartz orientation show
71	14		(4-3)-2-1-0	300 C_v quartz. Planes inclusions from same shown in figure 72.
72	14	126	8-6-4-2-1-0	Quartz orientation show
73	19		4-3-2-1-0	429 C_v quartz. Planes inclusions from same shown in figure 74.
74	19	150	25-20-15-10-5-2-0	Quartz orientation show
75	31		(6-5)-4-3-2-1-0	173 C_v quartz. Planes inclusions from same shown in figure 76.
76	31	50	22-16-10-4-2-0	Quartz orientation show

Remarks
Planes of liquid n same thin sections e 66.
ion shown in figure 65
Planes of liquid n same thin sections e 68.
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Planes of liquid n same thin sections e 70.
ion shown in figure 69
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RELATION

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maximum at *c* and a girdle about the *b* fabric axis. This gives a pronounced linear structure on the foliation (*s*) plane parallel to *b* because the traces of mica plates on a surface parallel to *s* are statistically parallel to *b*. (2) This type is the result of orientation of elongated amphibole or tourmaline crystals and has been observed to be parallel to the fold axes of small folds throughout the area. (3) The third type is caused by pinching and swelling of alternate layers of different mineral composition (see p. 344). This is a gross lineation best seen at a considerable distance from the outcrop and is parallel to the fold axis of the small folds. (4) The fourth type is the result of parallel orientation of the long axes of elongate inclusions. These are also parallel to the fold axes of small folds.

A fifth type is seen in hand specimens as striae on cleavage surfaces. The striae are perpendicular to the four fold-axis types of lineation and hence are believed to lie in *a* of the small folds.

In addition to the above, several younger linear structures occur, with variable relations with the fold-axis types. For example, one schistose horizon in the granite gneiss commonly has numerous small drag folds (see p. 347) that are obviously later than the regional foliation. The fold axes of these small crenulations can be easily measured as a linear structure. Also, small but easily visible striae, which are perpendicular to the fold axes of these drag folds, can be seen at numerous localities.

RELATION OF PLANES OF LIQUID INCLUSIONS TO MAJOR STRUCTURES

The major structures as observed in the field are illustrated in figures 27–29. The foliation strikes predominantly north-south and dips steeply to the west. This is, perhaps, best illustrated in the compilation of all measured values shown in figure 25 where lineation measurements are shown by dashed lines and poles to foliation by solid lines. The linear structures shown in figure 25 all lie in the plane of the foliation, and all those values illustrated are believed to be parallel to the fold axes of minor folds seen in several localities in the area. Therefore, they are believed to represent lineation parallel to *b* of the small folds. Figure 25 is a compilation of one hundred and fifty-one measurements of strike and pitch of linear structures.

The linear structures generally pitch south to southwest in the eastern part of the area, whereas they pitch northwest in the western part of the area. The change from southwest to northwest pitch is gradual in the northern portion and surprisingly abrupt in the southern part. An explanation of the orientation of the linear structures will doubtless be obvious when the relations to similar structures over a much larger area are known.

The orientation of the planes of inclusions is illustrated in figure 30. Each orientation shown is the orientation of the maxima from a contoured fabric diagram. The planes of inclusions strike generally northwest and dip to the northeast. Figure 26 illustrates a compilation of all maxima, and it can be seen that they are highly oriented (maxima up to 23 per cent in a 1 per cent area) throughout the area. In fact, they are more uniform in orientation than either the foliation (16 per cent maxima) or the lineation (8 per cent maxima). The planes of liquid inclusions cut across the primary structures (foliation and lineation) and bear no symmetrical relation to them. This in itself would indicate that the planes of inclusions are later than the

foliation and lineation and therefore are not genetically related.

It must be emphasized that any one of the maxima shown in figure 30 may be in error by as much as 20° because of the many manipulations that must be made in determining the orientation (i.e., orientation of hand specimen, orientation of thin sections, rotation of diagrams, etc.); however, in most cases it is believed that the errors are not more than $\pm 5°$. It is believed that the orientation of the planes may be considerably more uniform than is illustrated because, when special precautions were taken to eliminate errors, no differences in orientation could be recognized between hand specimens from the same outcrop. This is illustrated in figure 7.

In considering the distribution of the planes of inclusions, it is remarkable that no macroscopic structures could be found that were related to the planes. One would think that an orogenic stress that could produce such a structure throughout an area of 200 square miles would manifest itself in some other way. After the orientation of the planes throughout the area was known, considerable time was spent in the field revisiting many outcrops in an effort to find some macroscopic structure that could be related to the planes. None was discovered. It is hoped that workers in other areas, where auxiliary structures may be developed, will be able to make some correlation.

The relation of the quartz orientation to the planes of inclusions is illustrated in figures 65–76. As might be expected, there is no consistent relation between the two, since the planes of inclusions merely indicate fracturing. Such an environment could hardly be expected to reorient the quartz. The quartz orientation is in most cases related to the primary foliation (*s*) and lineation (*b*). This again indicates that the planes of inclusions are later than the primary regional structures.

LATE STRUCTURAL HISTORY OF THE WASHINGTON, D.C., AREA

After the metamorphism that produced the foliation and quartz orientation, the entire region was subjected to orogenic movements that induced fracturing in the quartz grains of all the rock types. These fractures were strongly developed in a northwest-southeast direction, with a steep eastward dip. The fractures were filled with solution, either interstitial or hydrothermal, which subsequently became trapped as minute liquid inclusions in the quartz. The initial thin layer of liquid along each fracture became disseminated into numerous isolated inclusions by a process of solution and deposition of silica. This process of solution and deposition apparently continued for a relatively long period, since the liquid inclusions had begun to develop crystallographic planes as boundaries before the next orogenic disturbance. The next orogenic disturbance manifested itself as numerous small drag folds in the less competent horizons of the area. During the drag folding the quartz was locally reoriented in part and deformed subparallel to the shear directions of the deforming stress as deformation lamellae (see p. 348). The lamellae are believed to have formed while the folding was in process, as they occur as a partial girdle about the fold axis. After this period of deformation the quartz was again fractured locally, and again liquid filled the fractures. These latest fractures still remain as thin sheetlike liquid inclusions, indicating that they are relatively young compared to the other structural features described.

Sometime during the above-described

events quartz veins and pegmatites were introduced. They are, in part at least, later than the foliation; but otherwise no evidence has been discovered to fit them more closely into the sequence of structural events.

GEOLOGICAL SIGNIFICANCE OF PLANES OF LIQUID INCLUSIONS

GENERAL

Planes of liquid inclusions have not been identified megascopically, nor have they been related to any megascopic structures, such as joints and fractures. The commonly described *ac* and *bc* fractures, both of which can usually, if not always, be recognized in the field, have not been recognized in the Washington, D.C., area; therefore, it is not possible to compare them directly with the planes of liquid inclusions. The most important distinction between the fractures and planes of liquid inclusions is that the latter are confined to the quartz grains of the rock, whereas joints and fractures usually represent failure of all the minerals in the rock. It is possible that megascopic jointing and fracturing are a relatively shallow-zone manifestation of the same stress environment that produces planes of liquid inclusions at greater depths.

Heretofore the only suggestion of the orientation of the planes of liquid inclusions with respect to a direction in the deforming stress pattern has been the en echelon tension-like planes described on page 338 (see also fig. 6). These planes would seem to indicate that either the shear or the tensional direction in the deforming stress pattern may be favorable for the development of fractures that subsequently become planes of liquid inclusions. It has been mentioned (p. 340) that the poles to planes of liquid inclusions commonly lie in incomplete girdles. These may be perpendicular to the fold axes. The evidence for this belief, presented in the next section, is the relation of a set of extremely thin sheetlike planes of inclusions to small drag folds found in a schistose horizon in the granite gneiss.

PLANES OF LIQUID INCLUSIONS IN SMALL DRAG FOLDS

The drag folds are younger than the foliation, since the mica flakes are bent around the folds with no evidence of recrystallization. The planes of liquid inclusions previously described are rotated by this folding and therefore antedate it. The maximum in diagram *A* of figure 77 shows the orientation of these planes in the portion of the schist that has not been rotated. Only a few of these planes could be found in the rotated portion of the fold, but all were obviously rotated during the folding. These planes of inclusions are therefore older than the drag folds and are not genetically related. In addition to these planes, characterized by a broad plane of nearly equidimensional inclusions, another type is present in sections of the drag folds. This type is characterized by extremely thin planes of sheetlike inclusions. Diagrams *B* and *C* of figure 77 illustrate their orientation with respect to the drag folds. They are symmetrically related to the drag folds and are believed to be also genetically related. They are essentially in the shear direction of the conventional stress pattern. In diagram *B* of figure 77 most of the planes are perpendicular to the foliation, which is one of the shear directions of the drag fold. In diagram *C* of figure 77 most of the planes are again nearly perpendicular to the rotated foliation, but this is also a shear direction of the drag fold. In both *B* and *C* there are some planes of inclusions in both shear directions, but one of the directions pre-

dominates in each case, hence providing a possible explanation of why only single-maximum diagrams are found in most specimens in the area. The drag fold is composed of alternating layers of mica and quartz, and in all probability the movement which theoretically should have produced two equally dense maxima in *B* of figure 77 was partially taken up by the less competent mica layers when the shear was along the plane of the mica layers. It should be noted also that a few planes were found that were in the tensional direction in the drag folds.

Diagrams *B* and *C* are sketches of what is believed to be the correct interpretation of the orientation of the thin sheetlike planes. Conventional contoured diagrams were not prepared because only a few planes were found in the thin sections available. Measurement of these planes is extremely tedious because they can be seen only when nearly parallel to the microscope axis and only a few were found in carefully traversing several large thin sections.

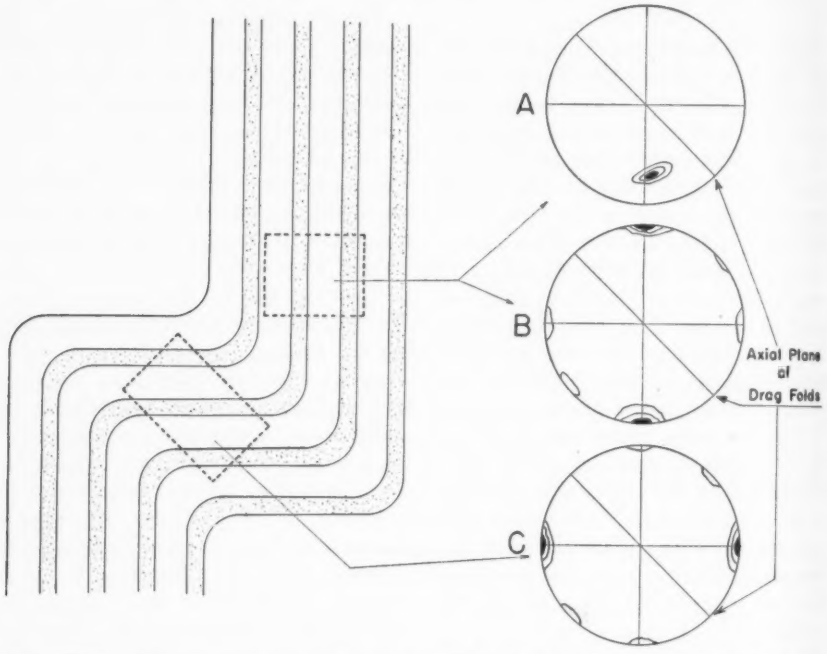

FIG. 77.—Diagrammatic cross section of drag fold, with schematic fabric diagrams illustrating the orientation of planes of liquid inclusions in the fold. *A*, poles to planes of liquid inclusions characterized by a broad plane of nearly equidimensional inclusions. *B–C*, poles to planes of liquid inclusions characterized by extremely thin planes of sheetlike inclusions.

RELATION BETWEEN DEFORMATION LAMELLAE AND PLANES OF LIQUID INCLUSIONS IN QUARTZ[2]

Deformation lamellae in the quartz grains of these drag folds have been described previously (Ingerson and Tuttle,

[2] Since the manuscript for this paper was prepared, an excellent paper has been published in this *Journal* (November, 1947) by N. Allen Riley, describing deformation lamellae and planes of inclusions from the Baraboo quartzite. His results

1948). The lamellae were found to occur in the shear directions of the deforming stress pattern, and it was shown that the orientation of the lamellae is unrelated to definite crystallographic directions.

Lamellae in quartz were first described in detail by Böhm (1883), and it is interesting to note that Böhm believed the lamellae to be planes of inclusions. Where the inclusions could not be seen, Böhm suggested that they were merely ultramicroscopic in size. A somewhat different explanation has been offered by other observers (Kalkowsky, 1878; Ingerson and Tuttle, 1948; and Fairbairn, 1941). Lamellae are believed to be an entirely different type of structure from planes of inclusions, and the confusion has been brought about by the fact that planes of inclusions commonly occur with the lamellae. Lamellae (pl. 1, *B*) are planar structures manifested by a slight difference in refractive index between the lamella and the surrounding area. In some cases the birefringence serves to distinguish their presence. They can be seen in thin section only when they are nearly parallel to the axis of the microscope. In many cases it is necessary to have sections several times the normal thickness to recognize them. They are not definite breaks in the quartz grains and do not pass uninterrupted from one grain to another of different orientation as do planes of liquid inclusions. In general, lamellae are more common within a zone from 10° to 30° of the base, although they have been observed in all positions from 0° to 90° of the base.

Lamellae have been interpreted as translation-gliding planes by some observers (Mugge, 1896; Sander, 1930). Fairbairn (1941) suggested that a glide line (*m*:*r*) was operative rather than one or more glide planes. Ingerson and Tuttle (1948) concluded that the lamellae are not controlled by definite crystallographic planes or zones. They suggested that the quartz had failed more nearly like an isotropic material than like a crystalline substance.

The orientation of the lamellae in the quartz of the drag folds previously described is illustrated in figure 78. The relation between the orientation of these lamellae and the planes of liquid inclusions can be seen by comparing with figure 77. Both structures are related to the shear direction of the deforming stress. This suggests that the two structures are produced in similar stress environments. The deformation lamellae tend to be less highly oriented than the planes of inclusions, indicating, perhaps, that they were formed at an earlier stage while the quartz grains were still being rotated by the deforming stress. Rotation of individual quartz grains did not occur during or after the fracturing that produced the planes of inclusions because a single plane can be seen crossing many quartz grains of different orientation without deflection at the boundaries.

RELATIONS BETWEEN PLANES OF LIQUID INCLUSIONS AND THE RIFT, GRAIN, AND HARDWAY OF GRANITES

Planes of liquid inclusions have been reported as being parallel to the rift, grain, or hardway of granites. Some investigators have reported parallelism between planes of inclusions and one of these directions, and others between planes of inclusions and two of these directions. In all examples the relation between the two structures was apparently

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are essentially in agreement with this work; i.e., deformation lamellae and planes of liquid inclusions are not related to macroscopic structures but are related to one another. Planes of liquid inclusions were found to have single strong maxima, two maxima, and partial girdles.

investigated by inspection of thin sections without the aid of the universal stage, so that orientation of the planes could not be accurately determined. For example, figure 79 (Dale, 1923, p. 18) is a camera lucida drawing of a section parallel to the hardway of a granite and is intended to illustrate that the planes of inclusions are parallel to the rift and grain. It can be stated with certainty that neither set of planes of inclusions illustrated is parallel to the rift or grain of the granite. The "strike" of the two sets of planes with respect to the thin-section surface is nearly parallel to the rift and grain directions, but the "dip" in both sets obviously does not coincide with the rift or grain in either case.

The interpretation placed on this example throws doubt on other observations made without the universal stage and emphasizes the need for careful statistical study of such structures. For example, planes of liquid inclusions are reported to be parallel to the rift of some pre-Cambrian granites of Quebec (Osborn, 1935). However, the method of determining the orientation is not given, and the observation may be subject to the same misinterpretation as that mentioned above.

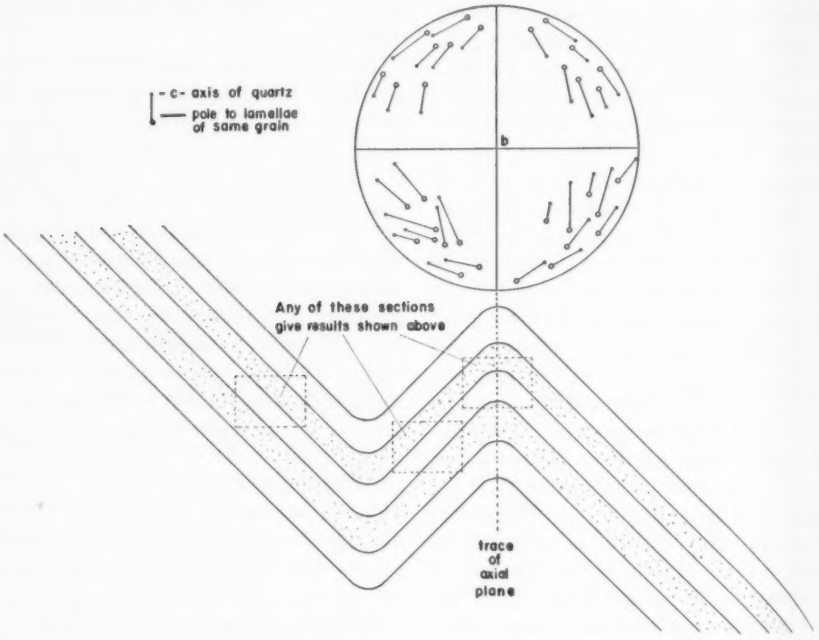

FIG. 78.—Diagram illustrating the relation between *c* axes, poles to lamellae, and fold axis of a small drag fold in mica schist. Note that the lamellae have the same orientation as the planes of liquid inclusions in fig. 77.

RELATION BETWEEN SHEAR AND TENSION FRACTURES AND PLANES OF LIQUID INCLUSIONS

Sander (1930) has studied and described the orientation of four types of fractures in quartz. Two sets (*ac* and *bc*) are tension, and two sets (intersecting in

a and *b*) are shear fractures. Unfortunately, detailed description of the fractures is unavailable to correlate them directly with planes of liquid inclusions. All four types are symmetrically related to the primary structures of the rocks and to the quartz orientation, whereas the planes of liquid inclusions considered here are not so related. At least two of the fracture types (*bc* and *b*) are megascopic structures, hence differing from the planes of inclusions in this respect. The four types of fractures have one important feature in common with planes of liquid inclusions, namely, the lack of control of their orientation by crystallographic directions in the quartz. This further emphasizes the isotropic nature of observable quartz failure in deformed rocks.

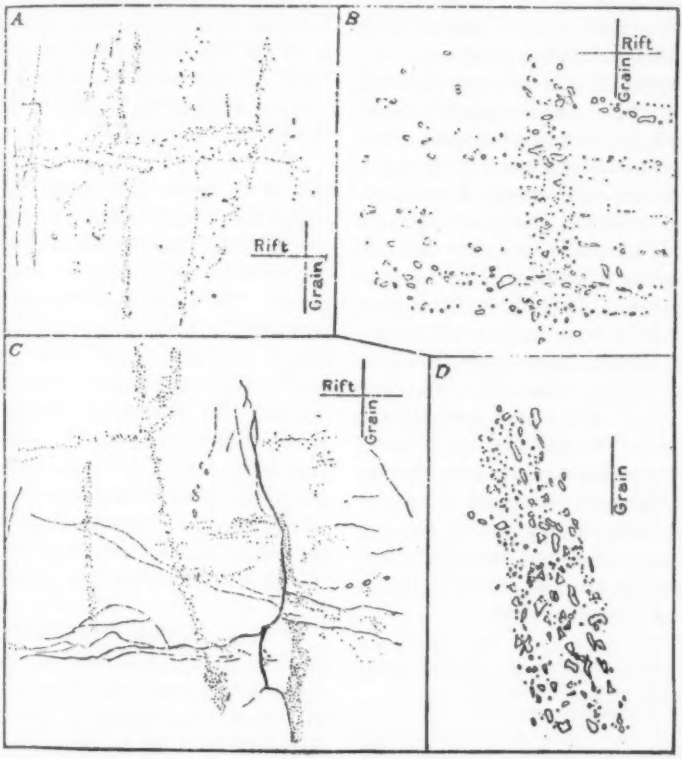

FIG. 79.—From Dale (1923): "Camera lucida drawings of quartz areas in an enlarged thin section of biotite granite from the Redstone quarry in Conway, N.H., cut parallel to the 'hard way.' *A*, enlarged 62½ diameters, shows sheets of cavities and incipient cracks in both rift and grain directions. *B*, from a different quartz area, enlarged 175 diameters, shows the variation in the size of the cavities and their arrangements in both rift and grain directions. *C*, from still another quartz area, enlarged 23½ diameters, shows conspicuous rift and grain cracks, filled with fibrous white mica, coinciding here and there with the sheets of cavities, also two parallel exceptional fractures crossing the rift and grain directions diagonally. *D* shows part of one of the grain sheets of cavities of *C*, enlarged 175 diameters in order to show the shapes of the cavities. As most of these sheets of cavities in the section appear to undulate in the direction of the line of vision when placed under the microscope the outlines of all the cavities become visible only by altering the focus."

ENVIRONMENT FOR PRODUCTION OF PLANES OF LIQUID INCLUSIONS

Planes of liquid inclusions have been described by Anderson (1945) as being related to crystallographic directions in quartz. The quartz studied by Anderson occurs in small veinlets in a quartzite and is believed to represent low-grade metamorphism, as it lacks secondary silica and contains much chlorite. The significance of this work is open to some question because only eight quartz grains were studied and statistical methods were not used. However, if it is assumed that the planes of liquid inclusions are related to crystallographic directions in some cases, then it seems reasonable to expect that a different stress environment is responsible for the control of fracturing by crystallographic planes in one case and the lack of control in another. The rocks in the Washington, D.C., area have been subjected to a very high-grade metamorphism and probably were buried to a greater depth than the low-rank chloritic rocks in the area investigated by Anderson. The speculation is therefore warranted that quartz may behave like an anisotropic material when deformed at relatively shallow depths, but at higher pressures (and temperatures?) and greater depths the crystallographic control of fracturing is lost.

PLANES OF LIQUID INCLUSIONS AND HYPOTHESES OF QUARTZ ORIENTATION IN DEFORMED ROCKS

Fairbairn (1942) has critically discussed three hypotheses of quartz orientation in tectonites. They are based on twinning, translation, and fracture. The twinning hypothesis is considered least likely to apply because little direct evidence has been obtained to substantiate such a mechanism. The translation hypothesis is also considered unproved because of the lack of direct experimental evidence. Deformation lamellae may originate by translation-gliding, but the lack of control (Ingerson and Tuttle, 1948) of their orientation by crystallographic directions in the quartz makes this hypothesis even less acceptable. The third hypothesis, based on the crystallographic control of fracturing, is considered most likely, and Fairbairn concluded (1939, p. 1490): "All the known quartz maxima in tectonites may be correlated with the fracture hypothesis . . ."; however, "the common oblique and *ac*-girdles are not adequately explained by any of the three hypotheses."

The fracture hypothesis requires quartz to break into needle-like slivers which are parallel to prominent crystallographic directions. The bounding faces of the needles must be crystallographic planes. The hypothesis is based on experimental deformation of quartz by Griggs and Bell (1938), who found that quartz broke into needle-like fragments bounded by prominent crystallographic planes when ruptured at elevated temperatures (450° C.) in the presence of sodium carbonate solutions. Quartz maxima are theoretically produced by rotation of the needles into shear planes with the prominent bounding plane parallel to *ab*.

It should be noted that the tendency of quartz to break into needle-like cleavage fragments was not encountered when the quartz was ruptured "dry" at room temperature with or without confining pressures. Thus the presence of solutions and the temperature may be important environmental factors controlling the behavior of quartz in tectonites.

The fracture hypothesis does not explain the ubiquitous girdles found in tectonites, and Billings (1942, p. 352) criticizes the hypothesis because ". . . we

should expect to find many rocks containing such slivers of quartz. Actually, they are very rare." The evidence presented here for the orientation of planes of liquid inclusions in quartz indicates that quartz fractures much like an isotropic material under the conditions prevailing during the development of the planes and fails to give evidence for crystallographically controlled fractures in quartz. This fracture isotropism is further emphasized by Sander's (1930) investigation of four different types of fractures in quartz. *All four types show no lattice control of their orientation.*

Thus the geological evidence of the isotropic nature of quartz failure appears to render the fracture hypothesis as untenable as the twinning and translation hypotheses. Some type of growth hypothesis may afford a more plausible explanation of quartz orientation where the environment does not permit lattice control of the failure of quartz. Such hypotheses have been proposed by several workers. Griggs and Bell (1938, p. 1745), for example, suggest that differential solubility and the effect of the stress field on recrystallization might be operative to preserve certain orientations and obliterate others. Cloos (1946, p. 39) has suggested growth to explain quartz orientations in primary igneous and metamorphic rocks. As quartz is the latest mineral to crystallize, its orientation may be controlled by the pre-existing mica, feldspar, and hornblende. Cloos writes: "Growth in a girdle plane may be in any rhombohedron or prism—a large array of possibilities, the only restriction being that the availability of many other minerals in a definite direction exerts an influence in the growth and orientation of quartz."

In conclusion, no completely satisfactory hypothesis of quartz orientation is known. Until quartz fabrics are synthesized and the orientation processes are better investigated, this state of affairs will probably continue.

PLANES OF LIQUID INCLUSIONS AS EVIDENCE FOR OPENINGS FOR HYDROTHERMAL SOLUTIONS

Examination of a thin section of one granite (locality unknown) shows two sets of planes of liquid inclusions that differ in the character of the liquid. One set contains inclusions consisting of three phases—vapor, water, and liquid carbon dioxide—whereas the other set apparently contains only a vapor bubble and water. It is obvious that the two sets are of different age and that the character of the liquid available to fill the fractures changed with time. It is difficult to see how the composition of the liquid available to fill the fractures could change without introduction of material; therefore, a hydrothermal source for one or both of the liquids is suggested. This introduces the possibility that the fractures may provide avenues for migration of hydrothermal solutions.

At first thought it would appear ridiculous to postulate that such minute fractures could provide channelways for any considerable amount of solution; however, when a specimen such as no. 33 is examined and as many as two hundred planes are crossed in a traverse of only 1 cm., it seems that the openings provided are worthy of consideration by geologists studying ore deposits. It should perhaps be emphasized here that no megascopic evidence has been discovered that will indicate the presence or orientation of the planes. Also evidence of movement along the fractures has not been observed, although slight displacements would be difficult to establish.

SUMMARY

Planes of liquid inclusions are common in the quartz of igneous and metamorphic rocks. The planes consist of numerous individual inclusions, which are commonly planar parallel to the plane. The liquid inclusions are believed to have originated as fractures in the quartz grains, which subsequently became filled with solution. The solution-filled fracture is believed to change to numerous individual inclusions by a process of solution and deposition of silica. The process may have originated by a mechanism similar to that discussed by Riecke (1895), which requires that solution take place in a stressed crystal in the area under greatest stress and precipitate in the area of least stress. Temperature oscillation would also be a contributing factor, as would the fact that an irregular inclusion does not represent the lowest possible energy level; hence the process of solution and deposition will continue until euhedral cavities are formed.

This constant change in the character of the planes of liquid inclusions suggests a method of estimating the relative ages of two or more sets of planes. "Young" planes are characterized by their sheetlike inclusions, whereas "old" planes are composed of inclusions that are more nearly equant and are bounded, in part, by crystal planes. The relative age of two or more sets could be determined also by many of the methods used to establish the relative age between two or more sets of faults or joints.

Statistical studies of planes of liquid inclusions indicate that there is no control of the orientation of the planes by crystallographic directions in the quartz, suggesting that the quartz behaved like an isotropic material. Some evidence was found suggesting that the planes may be developed in the shear or tensional direction of the deforming stress pattern.

Statistical representation of the poles of planes of inclusions on a Schmidt equal-area net gives diagrams that have maxima up to 45 per cent. Maxima of 20 per cent are very common. Strong single maxima are most common, but split maxima have been found repeatedly. In the Washington, D.C., area two or more distinct maxima occur in the quartz veins but not in the gneisses. In other areas two or more maxima are apparently common. Many fabric diagrams of planes of liquid inclusions have incomplete girdles that are believed to be oriented about the *b* axes. Occasionally complete girdles are found.

Planes of liquid inclusions have a remarkably uniform orientation throughout the Washington, D.C., area, striking northwest and dipping steeply to the northeast throughout an area of 15 × 20 miles. Primary structures, such as contacts, foliation, and banding, strike predominantly north and dip steeply to the west. This nonsymmetrical relation is believed to indicate that the planes of inclusions are younger than the primary structures and therefore are not genetically related. No macroscopic structures could be found that were genetically related to them, although it is possible that some of the quartz veins are so related. The quartz orientation is, in general, believed to be genetically related to the primary foliation and lineation.

Detailed study of small drag folds indicates that planes of inclusions may develop in the shear direction of the deformation but that one set of planes parallel to one shear direction predominates.

Quartz deformation lamellae are present in the small drag folds. They also occur in the shear direction and are be-

lieved to be genetically related to the planes of liquid inclusions in the same folds. As lamellae are susceptible to recrystallization, it is not surprising that they are not commonly found with planes of liquid inclusions.

One investigator has described planes of inclusions that are believed to be related to crystallographic directions in quartz. However, the material studied may represent deformation at relatively shallow depths and at low temperatures. If this is the correct interpretation, then planes of liquid inclusions that are not related to crystallographic directions may represent an environment characterized by high hydrostatic pressure and probably also high temperatures.

A study of planes of liquid inclusions may throw some light on the mechanism of quartz orientation in tectonites. It has been established that quartz fails like an isotropic material in certain environments and that those theories of quartz orientation based on crystal structure control of failure are not applicable in such environments. Orientation by some growth process would appear to be more plausible.

Quartz may fail by shear or tensional rupture and in certain cases by deformation, which produces lamellae that may be a type of translation gliding. Failure by rupture along crystallographic directions has been noted by one observer and has been produced experimentally in the laboratory. These different phenomena are believed to represent failure under varying environmental conditions, and the optimum development of any one type of failure depends on such factors as confining pressure, temperature, composition, and the presence (and character) of solutions.

As many as two hundred planes of liquid inclusions have been found in a 1-cm. traverse of a thin section of a quartzite. Although the openings are minute, their abundance suggests that they are worthy of consideration as a channelway for the movement of gaseous and other solutions.

ACKNOWLEDGMENTS.—Thanks are especially due Professor W. H. Newhouse for encouraging a study of liquid inclusions and for frequent discussion of the many problems encountered during this work.

Professor H. W. Fairbairn has had a continuing interest in this work and has made numerous helpful suggestions, for which I am grateful. The thin sections studied from areas other than Washington, D.C., were kindly made available by Professor Fairbairn.

To Dr. Earl Ingerson go my thanks for independently checking the personal element in preparing fabric diagrams of planes of liquid inclusions. He kindly spent considerable time preparing diagrams of different thin sections, which, I am pleased to report, gave results identical with mine.

Finally, I should like to express my gratitude to my wife for assistance in contouring fabric diagrams.

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ON THE GRANITE PROBLEM[1]

R. PERRIN[2] AND M. ROUBAULT[3]

ABSTRACT

Anomalies in the order of crystallization, the inadequacy of hypothetical aqueous residual and "pore" liquids, the complexities and inconsistencies of the theories of differentiation, the insuperable problem of granite emplacement (the "space problem"), the problem of the gneissic zones and the gradation of gneiss into granite, are all taken as evidence that the theory that granite is a product of crystallization of a magma is inadequate.

Evidence that granite originated by diffusion and reaction in the solid state is presented. Supporting this concept are the observed feldspathization of inclusions and country rock, double and reciprocal inclusions, digestion of transgressive dikes by the host rock, failure of "stoped" blocks to settle, microscopic evidence of replacement and corrosion, and the incomplete or arrested state of alteration of inclusions.

The eruptive character of granite dikes is an anthropomorphic or subjective impression. Conclusive evidence that some dikes are replacement phenomena is presented.

Though some authors admit granite to be polygenetic, it seems more probable that such a singular rock should be of one mode of origin, namely, diffusion and metasomatism in the solid state.

INTRODUCTION

The granite problem is again in the limelight. For many years the magmatic origin of granite was accepted without dissent; but now this dogma is being questioned by "metamorphists." There is vigorous defense by the "magmatists" against what Niggli (1942, p. 17) has called a revival of the "Universal Theory" without any new evidence or argument in support of it. In the past few years several authors have considered this problem. We appear to have been the first (1937, 1939), followed by Niggli (1942), Reinhard (1943), Read (1943–1944), and Raguin (1946). In the latest edition of *Eruptive Rocks*, Shand (1947) also presents a number of arguments for the classical theories.[4]

In this discussion we seem to represent the most liberal element, with Read following closely, whereas Reinhard, Raguin, and especially Niggli, support the more conservative views. Most petrographers in their writings have chosen one or the other theory. Some, on the other hand, (as, for example, Reinhard), have admitted that granite may be polygenetic.

The discussion has become more and more heated, and occasionally personal. P. Niggli (1942, p. 15) is particularly severe with R. W. van Bemmelen, who was the first to give full support to our ideas. Niggli makes no reference to our publications and therefore does not directly discuss our arguments. Nevertheless, he asserts that reactions in the solid state obtained in the laboratory are of little importance. He asks how the "emanations" were observed in the field and under the microscope, and why they rise. Finally, he asks what mysterious force tends to transform the sediments into a final product which is always the same, granite or granodiorite.

In a memoir which is itself not free of

[1] Manuscript received September 27, 1948.

[2] Administrateur-Directeur Général de la Société d'Électrochimie, d'Électro-métallurgie et des Aciéries électriques d'Ugine (Savoie-Alpes).

[3] Professeur, Université de Nancy, France.

[4] Just after this article was completed, we received Memoir 28 of the Geological Society of America, "Origin of Granite" (conference at meeting of the Geological Society of America held in Ottawa, Canada, December 30, 1947). Our comments on this important work are appended at the close of this paper (p. 376).

contradictions, Max Reinhard (1943, p. 34) is clearly disdainful of "a few unpleasing phenomena" (*einige unerfreuliche Erscheinungen*). He concludes, after praising Niggli: "Je geringeres Gewicht man ihnen beimist, desto weniger werden sie die notwendigen Auseinandersetzungen über das Problem der Granitbildung belasten." On the other hand, Shand, though a "magmatist," does not agree with him, writing us recently: "Your contribution to the study is a most important one, and it will teach all of us to observe more closely and think more clearly." More recently Barth (1939; 1947, p. 181) cited our works in terms whose courtesy we thoroughly appreciate.

The importance of reactions in the solid state and their role in granitization are viewed in completely contrary ways. However, these contradictory judgments so positively expressed do not lessen the general impression derived from reading the works of contemporary petrographers, that most of them are obviously disturbed by the facts disclosed and the deductions drawn from them. However, many are evidently still influenced by the "magmatist" ideas that were taught them or which they themselves are teaching.

Diffusion and reactions in the solid state are unfamiliar to most petrographers. On the other hand, the prevailing concepts of magmatic differentiation appear consistent with data presented in the solidification diagrams. The classical hypothesis about the influence of water, "pneumatolysis," etc., taught as a synthetic, apparently coherent, construction, looks like a scientific certainty. For this reason it is necessary to review the whole question and to outline, very briefly, the hypotheses on which the magmatic theories rest.

THE "MAGMATIST" HYPOTHESES

SEQUENCES AND ORDERS OF CRYSTALLIZATION

Rosenbusch has stated what is still considered the normal order of crystallization of elements of eruptive rocks and, consequently, of granite. According to him, the first to crystallize and therefore the first to be deposited in the theory of solidification of granitic magmas are zircon, apatite, rutile, and sphene. All these are uncommon constituents of rocks and therefore of the original mixture. The early crystallization of rare constituents is inconsistent with what is known of melts in general: rare constituents crystallize from a solution only if there is a break in miscibility. However, zircon (zirconium silicate) and rutile (titanium oxide) are quite miscible in silicate melts and can be used in large quantities in metallurgical slags (whose nature is like that of rocks). This fact, in itself, is rather disturbing

After the so-called "accessory" minerals just referred to, there should appear orthosilicates with biotite, metasilicates, calcoalkalic feldspars, and, lastly, silica. Therefore, the deposition of potash occurs first during the formation of biotite, is interrupted by crystallization of the plagioclases, and is resumed during the crystallization of orthoclase or microcline. Such an interruption of deposition of an element in this way is contrary to what is known about crystallizing solutions.

A much more striking example of the interruption in deposition of a substance is that of water. During the crystallization of the magma, water appears as a constituent in early-formed minerals—biotite and, in many cases, hornblende, It is absent from the plagioclases, orthoclase, and quartz but reappears, at least theoretically, with the residual solutions

(*rest Lösungen*) of Niggli. However, this water is rarely found in any mineral formed from such solutions. An interruption of deposition of this type is abnormal and even improbable. S. J. Shand has noted this anomaly (1944).

In a granitic magma (one with an excess of silica) it is not the silica that is thought to be deposited first. However, in the known silicate melts with an excess of silica, the silica is deposited first. How does one explain this anomaly? The usual concept is that water is in some way responsible. Assuming this and granting even the most improbable singularities, observation shows that, even from magmas of almost identical compositions, the order of crystallization may not be the same. We have cited (1939, p. 60) some of the numerous exceptions to Rosenbusch's rule. Shand, in the third edition of *Eruptive Rocks*, wrote:

> The only conclusion that we can draw from the foregoing discussion is that there is no such thing as a constant order of crystallization, applicable to all magmas or even to one magma under different physical conditions. The Rosenbusch rule has no theoretical or experimental justification, and even on the basis of observation it cannot be upheld. Yet the strange fact remains that the rule seems to be applicable to a large number of rocks. How can this be?

What hypothesis, then, can we formulate which will reconcile with physical laws of solidification these two numerous exceptions to the rule? Must we consider, as Niggli did, some mysterious "automorphoses" by the residual solutions of the magma? It would fail to explain the deposition of hornblende or olivine last in granites, a fact which led Sederholm (1923–1926) to the hypothesis of an ultra-basic residual magma, which is quite out of harmony with the general rule.

The formation of zoned feldspars with a sodic center or with alternating zones is not the least of the peculiarities of the crystallization of magmas. Several authors, including Phemister, Hil's, and Hibsch (Perrin and Roubault, 1939, pp. 63–65) have attempted to explain it. Norman L. Bowen, author of the anorthite-albite diagram, postulated a veritable dance of the feldspars in incomplete decantation in zones of magma with different temperatures (1928, p. 275). However, such an imaginative and unfounded hypothesis is inapplicable in the observed case of so-called "inverse zonation" (labradorite in the center and anorthite at the periphery), which is in absolute contradiction to the experimental diagram established by Bowen himself.

THE ROLE OF WATER AND RESIDUAL SOLUTIONS

Leaving out of consideration the water of quartz inclusions or of minerals such as mica, it is generally believed that water (or at least hydrogen) is necessary for the formation of granite (Perrin and Roubault, 1939, pp. 123–124). A considerable quantity is needed to explain the relatively low temperature of granite formation (in agreement with Goranson's experimental results). This water must disappear without producing the slightest distortion of the rocks by volume changes.

Granite must have lost its water, despite the very high pressures and relative impermeability of the rocks overlying the magmatic reservoir. On the other hand, lavas of the same composition arrive at the surface with 8 per cent water, which they retain, so that they solidify practically "saturated" with water (if Goranson's experiments are a valid guide to the solubility of water in lava).

Water plays a no less mysterious part

in the explanation of the "palingenesis" of solidified granites, i.e., those transformed on the spot, according to Sederholm, into new granites. These granites, having solidified and therefore lost their water, must now reabsorb water to account for their remelting at a necessarily low temperature. Every palingenetic stage requires the entrance and loss of water, again without leaving any evidence of deformation

The introduction of water quickly followed by its disappearance, which is needed to explain the lowered melting point of granite, is even more necessary to account for "residual" pegmatites, which, according to theory, were liquid at still lower temperatures What light do Goranson's experiments with alkaline quartz-feldspar mixtures shed on this question? Pegmatites should present fewer difficulties than the fusion of granite because both temperature and pressure were much lower. J. G. Koenigsberger (1942, p. 89) has given us the answer: Goranson's experiments have failed to prove anything in that respect. What happens, then, to the theory of pneumatolitic sequences and the older hypothesis?

But let us return to the subject of the residual liquids. These solutions are regarded as powerful agents of impregnation and transformation of the enclosing rocks (Niggli, 1942, p. 34). How, then, account for the extreme irregularity of metamorphism about granite bodies? In cases where it is almost nonexistent (Roubault, 1935, p. 21), must we assume the absence of pneumatolitic and hydrothermal stages?

These same residual fluids with their great impregnating power also have an equally important dissolving power over previously deposited minerals (Niggli, 1942, p. 21). This hypothesis championed by Niggli has been rightly attacked by several authors, including Bowen and Barth, who have shown that a residual liquid cannot dissolve previously formed crystals—it can only transform them into crystals with which it is in equilibrium.

It is an observed fact that pegmatite veins take on forms implying "corrosion" of enclosing rocks containing hornblende or biotite (early-formed crystals which ought not to dissolve), which makes it necessary to reaffirm the dissolving power of the liquid. Niggli invokes the "reaction principle" (1942, p. 21), which, however, does not permit a biotite or a muscovite granite to dissolve pyroxene or hornblende because of the antecedent position of these minerals in the reaction series. Such granite is "saturated" with respect to these minerals. Production of such crystals by the assimilation of sedimentary rocks is also not permissible. Eskola (1939, p. 379) writes that, in the light of Bowen's reaction principle, the granitization of limestone is generally rather unlikely. Yet the transformation of limestones by granitic endomorphism into real diorites is a widely known phenomenon, as Lacroix (1899) showed in his classic memoir on the granite of the Massif de Querigut in the Pyrenees. What new hypothesis should be formulated to explain this granitic endomorphism?

DIFFERENTIATION

The only scientific and logical theory published to explain the genesis of the so-called "differentiated" rocks from magmas is that of Bowen (1928). It is based on gravitative settling of heavy minerals, especially ferromagnesian, during the crystallization of the magma, and also upon a "squeezing-out" or filter-press action which separates the liquid parts from the already formed crystals.

Bowen's idea rests on an experimental basis. Niggli (1942, p. 71) believes that the criticisms of Bowen's theory are unjustified because they are concerned with the letter of the theory He amended it in his theory of the "complex gravitation crystallization differentiation" (*komplexe Gravitation Kristallization Differentiation*), according to which the first-formed crystals descend into warmer zones and redissolve, thus serving as vehicles for the transport of molecules. To this are added equalizing movements (*Ausgleichwanderungen*) and separations (*Abquetschungen*) by pressure. In like manner, according to Niggli, the assimilation is chiefly accomplished by the sinking of most of the blocks separated by "magmatic stoping." Niggli adds that we must explain not only how differentiation comes about but also how it leads to the formation of different, separated, partial magmas which are comparatively homogeneous in themselves. The mechanisms postulated by Niggli lead us to expect complete gradation between magmas and the rocks formed from them, but Niggli thinks that these mechanisms surely lead to relatively homogeneous partial magmas. He does not give reasons for this "surely," which, to us, seems hazardous. But in some so-called "differentiated systems," basic rocks overlie acid, which is contrary to theory and also to gravity. To interpret the relations on the basis of differentiation calls for new hypotheses involving squeezing, filter-pressing, or some other mechanism. In some places anorthosites overlie heavier gabbros (Nordingrå-Gebiet in Sweden; Barth, 1939, p. 95), which is to be expected under the differentiation theory, but elsewhere, as in the Adirondacks, the gabbros overlie anorthosites.

The formation of alkaline rocks (Barth, 1939, p. 91), the zoned rocks, such as the banded gabbros of Duluth, and the alternating zones of lherzolite and ariegite of the Pyrenees (Lacroix, 1899) offer special difficulties for the theories of magmatic differentiation. N. L. Bowen and Robert Balk have both offered hypotheses, but neither seems to us to be satisfactory.

At what point are we to stop? Let us repeat what Niggli has correctly said, that not only the differentiation but also the comparative homogeneity of the parts must be explained. The last case mentioned above is a good example of the problem to be solved by the "magmatists." Why not also attempt to explain the banded crystalline gneisses? Lest it be said that we exaggerate, let us point out that it is actually by differentiation followed by alpine dynamometamorphism that Casasopra (1939) explains the formation of certain gneisses of the Leventina Valley in Switzerland. These are identified by analysis as orthogneisses and also display very fine regular alternation of thin gneissic zones of various compositions.

THE EMPLACEMENT OF GRANITE

Although many "magmatists" regard assimilation of already existing rocks as of great importance, Niggli, carried along by the inflexible logic of his theories (which allow granite very weak assimilative powers), is led to regard most granites as the products of juvenile magmas. However, being fully aware of the difficulty of introducing such magmas into already prepared openings (1942, p. 56), he seems to adopt Eskola's ideas of partial anatexis. In addition, he postulates subsidence of blocks detached by magmatic stoping. More and more authors assign a large role to assimilation, but Bowen himself acknowledges that granitic magmas are incapable of large-scale

assimilation, and Raguin (1946, p. 194) says: "One would be tempted to invoke other, unknown, factors" ("On serait tenté d'invoquer d'autres facteurs inconnus").

The older work of the French school (Auguste Michel-Lévy, A. Lacroix, and Charles Barrois), together with that of Bucking and of more recent authors, such as Holmes and Reynolds (1947), have shown that in many cases the *mise en place* of so-called "eruptive granites" has taken place *à l'emporte piéces* without disturbing the sedimentary strata. In some cases traces of these strata have been found in exactly the position indicated by extension of the beds, either in the original form or as endomorphic modifications of the granite. What hypothesis will account for these relicts remaining in exact alignment within what was an eruptive liquid granite? These observations are of fundamental importance and are inconsistent with the "magmatists' " theory. Besides, the endomorphic parts of the granite are in many cases diorites or still more basic rocks. Theoretically, these rocks belonging to the eruptive rock facies went through a liquid stage, although, being anhydrous in the theory of differentiation, their melting point is much higher than that of watery granite. What new hypothesis must we create to explain this complete contradiction?

Another important point to which we shall refer later but which should be kept in mind has to do with the so-called "stoped" blocks. The theory of crystallization differentiation requires that crystals of chrysolite, pyroxene, hornblende, etc., the first to be formed in the solidification process, be decanted by gravity. However, Stokes's law demands that, with equal density and similar shapes, the rate of sinking should increase even faster than the increase in size of bodies. To explain the sinking of small crystals in a case where large or even great bodies located far within a granite mass (as in the Bergell area) have not descended may require another hypothesis. Only if the granite had a practically infinite viscosity—the viscosity of solids—can these observations be explained.

Related to the problem of granite emplacement is the emplacement of ultrabasic rocks: dunites, peridotites, and lherzolites, whose melting temperature at atmospheric pressure is very high (1,600°–1,800° C.). Here, too, hypotheses have been offered. However, Barth, who considered the matter at great length, unhesitatingly concluded that the problem is still far from solved (1939, p. 96).

ZONES OF ANATEXIS[5]

The emplacement phenomena briefly discussed above are more especially related to "eruptive" granites which clearly cut across country rock. It appears desirable to summarize some "magmatist" theories arising from the study of zones of anatexis.

It is not easy to understand the strict "magmatist's" theories of origin of the zones subjected to the action of Sederholm's ichor. The theories appear, in essence, to depend on the action of "residual pegmatitic solutions" capable of passing through all rocks, plus partial refusion—Holmquist's ultra-metamorphism and Eskola's partial anatexis imply such a selective fusion of the granitic elements, always with the aid of water. These theories are contrary to Wegmann's theory of transformations without fusion, by means of intergranular films. Eskola, however, seems to consider migmatization a sort of metasomatosis, but with the magma itself responsible

[5] Including the "metatekt" zones of Scheumann.

("das Magma selbst als Träger fungiert") (1939, p. 378). This refusion, thus admitted, leads to a new hypothesis: Most of the rocks may have gone through a molten stage without disturbance of the sedimentary laminations and the folding of the original layers (outlined mainly by residual micas). (It is these observations which led Wegmann—quite rightly in our opinion—to reject the presence of magmas in these zones.)

It is common knowledge that rocks which have always been considered magmatic (such as pegmatites or aplites) have been observed in "metatekt" zones, or even in higher metamorphic facies. They commonly occur as isolated masses, clouds, *trainées diffuses* ("schlieren"), veins, or veinlike bodies, without being visibly related to a deep-seated mass and without any possibility of injection. It is difficult to explain that isolation of rocks which supposedly formed from magmas at the end of "liquation." A new hypothesis—*Ausblütung*, or exudation—came from the ideas of ultra-metamorphism. Niggli (1942, p. 33), after quoting from the words of Holmquist, Scheumann, and others, imagines a "molecular dispersion phase" (*molekulardispersen Phase*), whose migration would mean a partial remelting of the rocks, accompanied by concentration of some of the constituents ("Das Einspritzen ist oft nur ein Sammeln und ein Wandern"). It is also noteworthy that Koenigsberger (1942, p. 89), who collaborated with Niggli, considered the formation of anatexic pegmatites from exudations impossible.

It is necessary to devise still another hypothesis to explain already formed cavities of extraordinary shapes in which the "exudation" products crystallized. Many of these are filled with feldspars only, and, as the feldspars are anhydrous and therefore have a high melting point, an explanation should be (but is not) forthcoming. In addition, what explanation is there of clays containing 5 per cent alkalies (Roubault, 1935, p. 253) found in contact with granite but without exudates? Likewise, how explain masses, veins, "schlieren," etc., filled with soda-lime feldspars and even ferromagnesian minerals (hornblende, micas, etc.) which are theoretically crystallized first and are not a part of the "light, liquid, portion" (*leicht flüssiger Rest und Teile*), gabbro pegmatite observed by A. Lacroix au Pallet, or pegmatites of nepheline syenite reported by Barth (1939, p. 97)? Indeed, the composition of such rocks bears no relation to theoretical "residual" solutions, Therefore, generally speaking, the classical theories concerning the water of granites of the "anatexic zones" and of pegmatites leads to a law: The less hydrated minerals a rock contains, the more water it has contained. This really demonstrates objectivity in adapting theories to observations!

CONTINUOUS GRADATION OF GRANITE TO GNEISS

This is, again, an old subject of discussion. H. H. Read (1944, p. 51), after noting Élie de Beaumont's opinion that the origins of granites, gneisses, and mica schists are related, adds (p. 52): "This profound statement has been forgotten by many a thoroughgoing magmatist. For example, Niggli [1942, p. 7] believes that the association of granite and gneiss *complicates* the granite problem. For some of us, on the other hand, this association may clarify the problem." Niggli says: "It is obvious that these passages can be interpreted in two ways: the proximity of the granitic magma has created favorable conditions for an 'ultrametamorphosis,' or the magma is but the end product of the 'ultrametamorphosis' "

("Es ist selbstverständlich, dass sich diese Übergänge zweifach deuten lassen: Die Nähe des granitischen Magmas schuf die für eine Ultrametamorphose günstigen Bedingungen order das granitische Magma ist nur das Endresultat der Ultrametamorphose").

The hypothesis of convergence of structure and of composition between solid rocks which have been metamorphosed (even if they have been partially melted) and rocks which have crystallized from a melted magma remains to be considered. Niggli makes no reference to the zones of completely chaotic mixtures of granites and gneisses of similar composition which are well known to petrographers. How can one conceive of some of them having been completely melted while others were not, when the points were so close that the temperature was necessarily the same?

Evidence has apparently recently compelled Niggli (1947, p. 153) to make some concessions to the "metamorphists" by distinguishing not only "magmagranites" and "ultra-metagranites" but also "metagranites" resulting from metamorphism without any fusion whatever. He thus presents the hypothesis of convergence.

In short, the "magmatists" have gradually built up a whole series of hypotheses (we have listed over twenty, and there are many others) which they require to explain the facts. But when a concept needs such a concourse of hypotheses to support it (many of which are in opposition to one another), we can properly conclude that, however pleasant the structure may be to the eye, the foundation is weak.

Moreover, as we have seen above, this accumulation of hypotheses leads the "magmatists" to postulate several different geneses for granite, the choice depending on the requirements of the case. Reinhard, like Niggli, admits of three—a priori, this fact alone should have brought doubts to the minds of these authors, for granite is so special a rock that it seems unreasonable even to consider the possibility of convergence. It is difficult for us to imagine that such a crystallization did not in every case suppose an environment of the same nature: solid *or* liquid—one or the other, but not both.[6]

No doubt we shall be told that we also venture a number of hypotheses, perhaps still weaker, in our theory of granitization in the solid state. Nevertheless, in the following section we shall endeavor to show that our theories are based on indisputable observational data.

EVIDENCE OF ORIGIN OF GRANITE BY DIFFUSION AND REACTIONS IN THE SOLID STATE

GENERAL REMARKS

The belief in magmatic granite began with Hutton's (the father of the plutonian school) observations of the eruptive aspect of some granites, their homogeneity, and their apophyses and veins. The differentiation-crystallization theory added in recent years only supports the conclusions derived from Hutton's original observations.

Thus the belief in melted granite arises, first of all, from a subjective impression. "Anthropomorphic" was the term used by Maillet and Pavans de Ceccaty (1937) to characterize a similar attitude in tectonics. These authors wrote further: "Tectonics is not the only discipline in which the sensory imagination should be neglected" ("La tectonique n'est pas la seule discipline où

[6] "Solid" is extended to include rocks previously discharged (*épanchées*) in the liquid state but solidified and, at a later period, affected by granitization.

l'imagination sensorielle doit être négligée"), and they showed the importance of logical reasoning, "the only instrument which can give access to these forbidden domains" ("le seul instrument qui puisse nous ouvrir l'accès de ces domaines interdits").

In tectonics the "anthropomorphic" necessity of a plastic state of rocks no longer exists, and many authors admit the "flowing" of mountains by the action of gravity. A similar mental attitude seems indispensable when considering the petrography of granular rocks; for the problem is to determine whether a subjective impression is to prevail over rational deductions drawn from indisputable observations. With this attitude of mind, let us examine the fundamental facts which justify our way of thinking.

NIGGLI'S OBJECTIONS

First, we think it is necessary to answer briefly Niggli's three objections, mentioned at the beginning of this article. The first concerns the difficulty of reactions in the solid state. Our present state of knowledge of chemistry makes a discussion of this point superfluous. Our earlier memoirs (1937, 1939) contained an extensive bibliography on the subject. Since then Jens A. W. Bugge (1946) and Hans Ramberg (1945), in particular, have shown anew the fundamental importance of these reactions. No petrographer should deny, therefore, that diffusion of ions in crystalline lattices is an experimentally proved fact. Industry daily makes use of such controlled reactions, as in the preparation of magnesium by the action *in vacuo* of silicon on dolomite.

Niggli asks whether emanations have been observed in the field and under the microscope and why they rise. We could answer that they were observed as well as the *molekulardispersen Phase* of Niggli and that they rise for the same reason. Professor Niggli apparently believes that it is normal for the pegmatitic solutions resulting from differentiation to rise (although he does not say how), but he asks, if the granite is itself the result of deeper emanations, where do they come from?

In reality, observation shows that the phenomenon is not restricted to a rising of elements. Close study of the borders of a granite body, with or without zones of "eruptive breccia" or of "magmatic stoping," and of the evolution of the inclusions (*enclaves*) in any granite shows that certain elements came in while others went out.

Referring to such zones, Niggli wrote (1942, p. 41) that it is difficult to decipher from the final product all the stages of formation, because what has been a successive series appears in juxtaposition (*das Nacheinander wie ein Nebeneinander erscheint*). We do not agree with him; anyone who has examined border zones knows that it is possible to find practically all stages of transformation in the different inclusions, from the original rock (completely or nearly intact) to rocks hardly distinguishable from granite. This was observed by Lacroix in the Pyrenees (1898–1900). Raguin writes, about a schist-granite contact, that one passes into the other by a series of steps ("du schiste au granite, il y a ainsi continuité par une série d'échantillons"), and he describes examples of intermediate zones between the enclosing rock and the granite.

Simple visual examination of the inclusions obviously indicates that this gradation usually is characterized by increase in the proportion of alkaline feldspars and a decrease of the ferromagnesian elements.

We have called attention to the impor-

tance of diffusions *à double sens* (1946*a*), and recently Holmes and Reynolds (1947, p. 55) have given a new and magnificent example of this phenomenon. Such diffusions seem to be a general phenomenon, observed not only in cases of granitization but also in basic rocks. In the latter case nonalkaline elements, such as alumina or iron oxide, are diffused toward the enclosing rocks (Tilley, 1947, p. 104; Lacroix, 1899). The phenomenon also seems to be characteristic of metamorphism generally (Lapadu-Hargues, 1945; Perrin and Roubault, 1946). Therefore, the assertion that granitization is accompanied by diffusions *à double sens* is proved by verified facts. It is unimportant that the reasons for these deep exchanges are not yet apparent; the facts are there and are incompatible with the theories of simple influx of residual solutions, pore solutions, etc. On the contrary, they are observed in the case of reactions in the solid state.

Niggli asks what mysterious force always tends to transform the sediments into the same type of rock—granite or granodiorite. Niggli's idea is clear: it is normal for magmatic differentiation to tend toward a single rock type, but not for metamorphism to do so. He criticizes Van Bemmelen and Backlund (1942, p. 17), who think that the type "granite" is the expression of a tendency toward homogenization, toward a state of equilibrium. Let us note, first, that there is a tendency, not toward a single chemical composition, but rather toward an average chemical composition. This is made obvious by the very existence of Atlantic, Pacific, and Mediterranean provinces, by the diversity of composition of crystalline massifs—in brief, by the mass of evidence which has led to the theory of differentiation. In spite of this, the tendency toward homogenization remains an observed fact: granites tend to alkalinize their inclusions and gabbros to basify theirs, etc.

Recently, Lapadu-Hargues (1945) investigated the composition of a great number of "silica-alumina" rocks, from unaltered sediments to mica schists, gneiss, and even granite. He plotted the percentage of alkalines in molecular proportions as the ordinate and the percentage of alumina as the abscissa. For each rock type he drew a line enclosing the representative points of the alkali/alumina ratio, then connected the average points of the zones. He found, first of all, that the areas over which the points were distributed became smaller as the degree or metamorphism increased—in other words, the dispersion of analyses of rocks of the same type from different countries of the world decreases as the intensity of metamorphism increases. Moreover, he found that no break occurs in the line at the boundary of the gneiss and the granites. Thus the tendency to homogenization is evident in general metamorphism; with the transformist theory in mind it does not seem extraordinary that this tendency would be maximized in granite.

It should not be forgotten that geophysicists consider the heterogeneity of the earth's crust as only superficial. Consequently, it is not unreasonable to believe that sedimentary rocks, derived by alteration of the primitive crust, poorer than the crust in alkaline constituents, are in chemical equilibrium neither among themselves nor with their substratum. If at some time proper thermodynamic conditions arose, we do not see what would prevent homogenization by diffusion of ions, with the rising of alkalies from below.

We shall now discuss results of incomplete homogenization, namely, the so-called "eruptive" systems formed pri-

marily from pre-existing detrital rocks. The world-wide abundance of granites and granodiorites testifies only to an ancient predominance of siliceous and silico-aluminous detrital rocks. This does not seem to us to be inconsistent with the existence of petrographic provinces. Let us now examine the principal facts which support our viewpoint.

THE FACTS

FELDSPATHIZATION OF THE COUNTRY ROCKS AND OF INCLUSIONS

Many authors have pointed out the existence outside of granite (either in the enclosing country rock, which indisputably remained solid, or in inclusions) of feldspars that are identical with those of the granite in every characteristic: composition, size, shape, color, inclusions, presence of microperthite, zonation (orthoclase surrounded by oligoclase, as in the rapakivi granite), etc. Large crystals of feldspar may even be found astride the granite contact. We have already dealt at length with this subject (1939, pp. 69 ff., pl. 1, fig. 15), taking into account the observations of other authors as well as our own. H. H. Read has independently cited similar examples. This extremely common observation is the basis of our ideas. Because forms of crystallization are so sensitive to chemical composition and environment, we concluded:

> The probability that crystals of feldspars, identical in every way, could have originated and grown under such radically different conditions as those of an acid liquid magma on the one hand, and of a solid rock (with a different composition, even mesocratic in some cases) on the other hand, is practically nil—and feldspars of a granite identical with those of the different, solid country rock, originated under the same conditions, that is to say, in the solid state.[7]

[7] "La probabilité pour que des cristaux de feldspaths, identiques en tous points, aient pu naître et se développer dans des conditions aussi radicalement différentes que celles de cristallisation dans un magma fondu acide, d'une part, et de cristallisation dans un milieu solide, de composition différente, allant jusqu'à des roches mésocrates, d'autre part, est pratiquement nulle et les feldspaths du granite, identiques à ceux des terrains étrangers solides, ont pris naissance dans les mêmes conditions, c'est-à-dire dans le solide."

In a less precise, but more pungent wording, Read takes up the argument:

> One can, of course, believe that the two sets of feldspars, though admittedly identical, are really formed in two quite different environments, one magmatic and the other solid rock. I am tempted to remark, as the Duke of Wellington did on another occasion, that if one can believe this, one can believe anything! If this is really true, in view of the profound differences in the environments, I feel that a great part of the lore about magmatic crystallization becomes meaningless. I consider it more reasonable, and in the ultimate more honest, to hold that there is only one environment, that of the solid country rock [1944, p. 86].

Niggli and Reinhard made no reference to these observations or to the deductions drawn from them. Only Raguin attempted an explanation (1946, p. 184) by calling on Fenner's hypothesis, according to which, ". . . if three phases, solid, liquid, and gaseous, are in equilibrium, the deposition of crystals out of the liquid should be accompanied by the deposition of the same crystals out of the gases" ("si trois phases, solide, liquide, gazeuse sont en équilibre, le dépôt de cristaux à partir du liquide doit s'accompagner du dépôt des mêmes cristaux à partir de vapeurs"). However, the same author very prudently wrote also:

> If there is, in cases of this sort, an objection to the passage of the granite through the liquid state, one may answer that, among the prevailing chemico-physical conditions, no one knows which determine the composition and the facies of the feldspar crystals formed. It is possible that, among these factors, the liquid state, more or less advanced, is not essential. At

the moment of crystallization of the feldspars, the granite is no longer liquid, but mixed, with numerous solid particles in suspension. Reciprocally, the surroundings are soaked in the solutions. The physical states are perhaps no longer very different.[8]

Here again Wellington's remark comes to our mind. So, before changing our opinion, we shall await an incontrovertible explanation of the phenomena of feldspathization based on the magmatic theory.

THE PHENOMENON OF DOUBLE AND RECIPROCAL INCLUSIONS

The formation of crystals in the country rock or in inclusions identical with those of the granite is not restricted to the alkaline feldspars, particularly orthoclase. Plagioclase and ferromagnesian minerals may occur in a similar manner; the examination of certain contacts gives the impression that granitization grows somewhat like a mold which invades the enclosing rocks little by little. There are either numerous isolated crystals on the bottom of the old rock corresponding to what we have called the "progression de la granitisation en cristaux isolés" (1939, p. 75) or actual granitic areas inside the rock (1939, pl. 1, photo 3). The two phenomena are closely associated. Read (1944, pp. 86–87) has reported similar observations.

The continuity of the phenomenon is demonstrated by microscopic examination. In the case of the granite of Trebeurden, Bretagne, we have observed crystals of orthoclase with quartz, or crystals of orthoclase associated with plagioclases (1939, pl. 3, microphoto 4). Finally, there occur masses of granite existing as *inclusions within the inclusions of the same granite*—it is to this phenomenon that we have given the name of "double inclusion" (*double enclave*).[9]

Also observed is granite which contains inclusions of the surrounding rock, and, reciprocally, this rock contains inclusions of granite. This we term the "phenomenon of reciprocal inclusions." We have given an example of this from Bénodet, Bretagne (1947), and analogous observations have been reported by Wenk (1943, p. 267) and Barth (1947, p. 178).

Some "magmatists" will be tempted here to invoke *Ausblütungen*, which, according to the proponents of this hypothesis, are mixtures with minimum melting points, the melting points having been lowered by water, which afterward disappeared. We have already pointed out that a number of hypotheses are needed to support *Ausblütung*. In the present case an explanation by means of *Ausblütung* with minimum melting point is impossible by reason of the observed continuity from the individual feldspar crystals (with a high melting point) to whole granitic bands. An indisputable geologic fact remains: there have existed, in rocks in contact with granite or enclosed within it, favored points where there were formed crystals identical with those of the granite, followed by the formation of whole granitic zones.

[8] "S'il y a, dans les faits de ce genre, une objection au passage du granite par l'état liquide, on peut répondre que, parmi les facteurs physico chimiques régnants, nul ne sait quels sont ceux qui déterminent la composition et le faciès des cristaux naissants de feldspaths. Il est possible que, parmi ces facteurs, l'état liquide plus ou moins avancé ne soit pas essentiel. Au moment de la cristallisation des feldspaths, le granite n'est plus liquide, mais 'mixte' avec de nombreuses particules en suspension. Réciproquement l'auréole est alors imbibée de microsolutions. Les états physiques ne sont peut-être plus très différents."

[9] Raguin (1946, p. 183) explains *double enclave* as the presence of separate crystals of orthoclase in inclusions of the granite; we consider that a simple case of feldspathization of the inclusions.

DIGESTION OF DIKES BY GRANITE

Dikes of several kinds, with sharp contacts and parallel edges (diabase, dolerite, aplite, granulite), intersect a granite, but in places some granite "apophyses" project into the dike or may even replace it entirely and retain a few widely separated and more or less transformed inclusions of the vein (Perrin and Roubault, 1939, pp. 127–129; 1947).

Sederholm (1923–1926) observed similar phenomena, which he ascribed to a "reviviscence" of the granite. The latter, after having been cut by the dike, would have remelted and been injected into some fissures opened in the dike. Raguin (1946, p. 19) reverts to this hypothesis in studying a dike which intersects an old granite, described by Sederholm. He adds: "A paradoxical fact, this granite is at the same time much older and much younger than this basalt" ("Fait paradoxal, ce granite est à la fois beaucoup plus vieux et beaucoup plus jeune que ce basalte"). It is simple and positive!

How did the granite melt again? How else except with the help of water because water is necessary for the refusion. The solidified granite all at once received a lot of water as the temperature rose; but, in spite of an implied large increase in volume, no mechanical strains resulted, as there is no evidence of deformation in the intact or unaltered portion of the dike. However, some fissures were opened, and the "revivified" granite was injected into these fractures; then the water went away, and all was normal again. The water must not have reached the dike or else (according to the classical theories) it would have been melted because its melting point ought to be lower than that of granite, even if it were aplite or granulite.

Unfortunately for the theory, in many cases there is not only the appearance of injection but actually of digestion—of magmatic stoping or complete digestion. In some examples even the edges of the dike remain unchanged in the digested parts (Perrin and Roubault, 1939, pl. III, fig. 3; 1947).

We shall not, for the time being, form a hypothesis concerning the genesis of the dikes; but, whatever the hypothesis, the contents of basalt or diabase dikes were injected in a liquid state. The related magma then intersected solid rock, and the granite was substituted in these necessarily solid rocks; the granite digested in places the earlier dikes as it digested the enclosing rock. This granitization was possible only in the solid state; for, in the midst of the melted granite, some of the dikes of aplite or granulite, theoretically more fusible, would have melted and been mixed into the granitic magma.

NONSETTLING OF INCLUSIONS

Inclusions, very common in contact zones, even some of great size and heavier than granite, have remained in place. All the theories of Bowen and Niggli are based on the settling of little crystals of olivine or amphibole; but these same authors do not find abnormal the failure of enormous inclusions to settle. Such an attitude leaves us amazed!

Some authors believe that the granite of border zones was extremely viscous. But we note that this granite, rich in pegmatitic solutions, cannot have high viscosity and, at the same time, the extreme fluidity necessary for the formation of lit-par-lit injections.

Observation shows that inclusions rich in "ferromagnesian" minerals are very common in the heart of granite masses. Some of them are *homoeogènes* and others *énallogènes*, according to Lacroix's terminology. For all of them the melting

point is (according to the "magmatist" theories) higher than that of the granite, and their density is also greater. Although located in zones where there was no exceptional cooling, they did not settle.

Lacroix and many others have emphasized the extreme slowness of the granitization processes. The failure of the inclusions to sink is compatible, therefore, only with a state of "infinite" viscosity—the viscosity of solids.

EVOLUTION OF CRYSTALLIZATION

We have considered, first, some fundamental field observations and then some equally basic details of crystallization of the minerals of granite and of associated rocks. Lack of space prevents discussion here of the phenomenon of corrosion between crystals (Perrin and Roubault, 1939, p. 160, pl. 5, microphotos 7 and 8). Other authors, in particular Shand (1942) and Bugge (1946), have made similar observations of this very common phenomenon.

We shall cite as an example only the plagioclase "islets," unquestionably "corroded," surrounded by quartz and arranged in such a manner that the twinning planes are rigorously parallel. In addition, twinning bands of the same width are clearly aligned from one "islet" to another. In such a case dissolution is out of the question. The new crystals were substituted for the old ones by the substitution of the new lattice for the old, and this was possible only by the diffusion of ions of alumina, sodium, and calcium, which diffused through the crystalline lattice of the quartz to become fixed at more or less distant points. The same is true of hornblende replacement by orthoclase (Perrin and Roubault, 1939, pl. 3, photos 6 and 7); some calcium, iron, and magnesium ions diffused through the crystalline lattice of the orthoclase, and potassium ions diffused in the opposite direction to nourish the orthoclase lattice. Here is unquestionable evidence of diffusion in two directions in a solid state—diffusions observable on the microscope stage.

Water or, more correctly, hydroxyl OH, is but a special case of the diffusion of hydrogen ions. Bugge (1946) insists that, even if the constituents of a hydrated mineral were substituted in such a fashion, the reaction should be considered as taking place in the solid state.

Even if the processes had been "set off" by gases, pore solutions, or intergranular films, microscopic observations demonstrate that diffusion of ions through the crystalline lattice was necessary. As an example, Shand (1942, p. 417) described "some plates of hornblende which enclosed the corroded but undissolved remnants of the three minerals [hypersthene, augite, and plagioclase]"—how else may we explain the transformation of a pyroxene fragment wholly enclosed by the hornblende?

Along the same line we have never understood how authors who invoke gases or pore solutions can thus explain the formation, in a solid rock, of a metacryst of orthoclase which replaces an aggregate of quartz, biotite, and plagioclase; replacement requires that iron, magnesium, and calcium have gone out and potassium come in. In the light of modern physicochemical knowledge we see only one possible explanation: diffusion in two directions through the lattice.

RATE OF GRANITIZATION

Along the borders of many granitic systems (Mont-Blanc, Pyrenees, Massif de Bergell, etc.) are found inclusions of the country rocks. Some of these inclusions, located very far from the contacts,

are at different stages of transformation. The very existence of incomplete transformation revealed by the study of these inclusions indicates extremely slow processes. These processes were certainly much slower than the known speed of homogenization in a liquid state. They were also much slower than the speed of solution of a silicate solid in a molten silicate bath. Let us recall here the perfect miscibility of silicates.

The slowness of reactions of diffusions in the solid state has been cited as an objection by some. To us it seems, on the contrary, indispensable to explain the facts, even in proved cases of diffusion over long distances. Taking into account the enormous duration of geologic time, experimentally determined speeds of reactions in the solid state would be much too great for the inclusions to survive.

When Bugge (1946, p. 48) writes: "The chief objections against such a long distance diffusion of ions through the crystals is the small rate of diffusion. But I do not think this is a deciding objection. The total development of a mountain range comprises a period 250–300 million years," we quite agree with him, but with a difference. This slowness is a favorable, rather than a negative, argument for our viewpoint.

THE ERUPTIVE CHARACTER OF GRANITE DIKES AND APOPHYSES[10]

The eruptive character, or "aspect," is the basis for the classical conception of melted granite; it is therefore normal that it should form the "anthropomorphic" objection to our conceptions. Following are some facts.

We have previously described (1939, p. 95) the occurrence in some crystalline granitic masses of crystals of feldspar or amphibole in what appear to be apophyses or veins with parallel edges, but of coarser grain than the crystalline massif itself. The injection of a liquid in a preexisting slit is out of the question. We have published photographs of this phenomenon (1939, pl. 1, photos 6 and 10) and have since observed new examples of the same thing on the Brittany coast (in the Argenton area, in Finistère).

We have also published observations on granite "veinlets" of the Anse de Sciottot, near Flamanville (1938*a*). Transverse sillimanite-rich zones in the granite dike appear to be but prolongations of such zones in the country rock, and they thus preclude the possibility of a liquid injection of the dike. A very fine and quite similar example has just been described by Bearth (1945). He figured a granite apophysis of the "Mont Rose" in clear discordance with the paragneiss and described some garnet beds of the gneiss which continue into the granite. He concludes that the granitization is thus the result of a metasomatic process or replacement.

No comment appears necessary. Injection of liquid is out of the question in this splendid example in which the garnet lines remained in place, just as zones of inclusions commonly do. We have in this example obvious proof of the emplacement of granite dikes by means of simple chemical exchange, to the exclusion of any injection.

The alignment of discrete and separated feldspar crystals, which we as well as Charles Barrois and Auguste Michel-Lévy have described (1939, pp. 96–100), is a related phenomenon. Did not Barrois

[10] Some authors (Wegmann, Green, Backlund, MacGregor and Wilson, and Reynolds) are quoted by Read in support of the hypothesis of "mobilization" to explain the eruptive aspect. We believe it unnecessary to envisage, for most cases, any major movement—in our opinion, and therefore in our analysis, the explanation of the eruptive aspect is different.

use the words *filons moniliformes* to characterize these peculiar alignments? After our first memoir we published a new study, with photographs (1938*b*), on the phenomena of magmatic stoping at St-Jean-du-Doigt and also observed another splendid example at Argenton, Finistère.

In all these cases, in order to explain such alignments by the hypothesis of liquid injection, we must imagine some mysterious fissures which would later heal over without leaving any evidence. Must we also, perhaps, admit of cavities prepared in anticipation of the injections? In the absence of such openings, what is the mysterious composition of a liquid which, by solution of hornblende and plagioclase, would lead to orthoclase? If we are not to search for an interpretation which would at any cost save the idea of liquid injection, this phenomenon must be considered, as being of the same nature as that responsible for feldspars in the rocks bordering granitic masses.

The feldspars found in the country rock are the crystal precursors of the normal granitization front; in like manner, the aligned feldspars are the precursor crystals of the apophyses or dikes of granite or pegmatite. *We must therefore accept the idea that, in some cases, granitization follows preferred directions; it results, therefore, in the construction of apophyses or dikes. The phenomenon is the same whether granite proper or pegmatite or aplite is in question.*[11]

ARE THERE DIFFERENT WAYS FOR THE GENESIS OF GRANITES AND ASSOCIATED GRANULAR ROCKS?

Taking the advice of Maillet and Pavans de Ceccaty (1937), we have tried to neglect *imagination sensorielle*, to leave out of account the *représentations concrètes* to which our senses are accustomed, and to outline a theory for the genesis of granites from the observation of incontestable facts. Have we the right to extend to all granites the conclusions which have forced themselves upon us?

Let us make it clear that we are concerned only with the process of granitization. We do not consider the fact that a granite can be formed by substitution from very different rocks—sedimentary, metamorphic, microlithic (such as lavas), or even other granites. Thus we do not exclude the possibility that some granites or some granular rocks could have been formed through mere recrystallization in a solid state of eruptive rocks. One of us (Roubault, 1935) has described aplite veins in Kabylie de Collo, where the petrographic relationship with the rhyolite veins of the same area is obvious. We think it quite possible that the recrystallization of veins whose contents were originally formed by a liquid lava has resulted in a granular rock of the same or very similar composition.

All the observations that we have made lead us only to the belief that some diffusion, however slight it may be, is necessary for crystallization into a true granite with coarse constituents. But the essential problem is to determine whether granitic texture always originates in the solid state. This is the crucial point.

An increasing number of geologists, at first incredulous, now admit the possibility of granitization in the solid state but are reluctant to admit its prevalence. They tend to hold the opinion of Read, who wrote: "There are granites and granites." This author does not deny the

[11] After considerable delay there has just come to our attention an article by H. G. Backlund (1943), in which we are pleased to find ideas convergent with our own. Wegmann's excellent observations (1943) concerning progressive granitization of the Amistock conglomerate in Greenland also adds new evidence favoring our thesis.

possibility of a magmatic origin for little granitic bodies whose emplacement is not an insoluble problem; on the other hand, he repudiates it for most granitic masses and especially the big ones.

As noted elsewhere, Max Reinhard, too, believes in the formation of granite without melting, by metasomatosis, but he also admits two other possible geneses: juvenile granites and palingenetic granites. Niggli (1942, p. 83) approvingly quotes Frank Grout's remarks:

> Recent writers have now suggested that igneous-looking rocks resulted from metasomatism in so many places that they assume the process is established, wholly fail to see the complexity of the problem, and quote each other as authorities, but not one area has been described convincingly. It seems that the enthusiastic advocates carry their idea too far, applying it to masses several miles across; the conservatives admit a few feet; the truth may lie between, but the conservatives have the weight of evidence on their side, and the burden of proof is on the advocates of extensive granitization.

Grout seems to have summarized the situation quite well.[12] The present conflict is epitomized by the following quotation from H. H. Read (1944, p. 86):

> The inability of the magmatists to interpret the plain field-facts arises from the inertia of a century of belief in the magmatic origin of all granitic rocks. The inertia has been increased for many by the addition of weighty physico-chemical arguments which "explained" the phenomena. It takes a certain amount of courage to go around to the other side of these problems and look at them from a completely different situation.

The weight of evidence claimed by Grout does not seem very heavy to Read. Having for thirteen years had the courage to differ from the authorities, we continue to behave as representatives of the "extreme left," not because we mischievously want to look like revolutionaries, but because we know of no fact which would justify our setting a boundary between the "few feet" and the "several miles"—and we do not want to say, out of caution, something that we do not believe.

At the risk of repeating ourselves, we think it necessary at this point to outline the granite problem as it appears from the writings of even the most "magmatist" authors. None of them (Niggli, Raguin, Grout, or Reinhard, for example) would think of denying that rocks indistinguishable from granites were formed in the solid state by metamorphism or metasomatism. Nor would they deny gradation from granite to alien rocks of sedimentary origin, in particular, crystalline schists, paragneiss, quartzite, etc. However, the "magmatists" consider that only border phenomena are in question; Grout's "few feet" are explained by the phenomenon (or, more correctly, the word) of "convergence," so they go on thinking of the interior as resulting from the crystallization of a liquid. Raguin (1946, p. 68) admits such a convergence between a granite with endomorphic alteration and its sides or its exomorphosed inclusions. The same convergence is envisaged even where the granite border has not been endomorphically altered. Grout, who does not deny the continuity, evokes the difficulty of proving metamorphic origin inside the massif. As to Reinhard, he admits the total structural convergence of metasomatic, juvenile, and palingenetic granites.

Behind all the words and reservations is the following important hypothesis: Similar granitic structures can result from processes as radically different as

[12] In 1941 Grout knew only one of our granite papers—the one about "magmatic stoping" of St-Jean-du-Doigt, which he quotes; accordingly, he makes no reference to our other observations and arguments.

crystallization from a liquid and transformation of a solid rock by means of chemical changes in the solid state. Niggli (1942, p. 60) and Reinhard (1943, p. 24) say so plainly: "Material transfer may result in structures convergent with magmatic structures; even if the metamorphism takes place in the solid state," and "Granite may be formed from sandstones and clays through such metasomatic-metamorphic processes without liquefaction. These granites do not differ from magmatic granites either in mineral composition or in fabric" ("Stoffwanderungen können zu Konvergenzen mit Metamorphose im festen Zustand erfolgte," and "Es können aus Sandsteinen und Tonen durch solche metasomatisch-metamorphe Prozesse ohne Enischmelzung Granite gebildet werden, die sich weder in Mineralbestand noch im Gefüge von magmatisch entstandenen Graniten unterscheiden"). The thought behind the words is obvious: "endomorphe" supposes a melted state, "exomorphe" a solid state. We wonder if these scientists realize the full implications of the hypotheses thus expressed, numerous singularities of granite structure and composition being considered; for in the two cases the characteristics are the same down to the slightest detail. To put it emphatically: *the probability of producing such similar structures by such different processes seems to us absolutely nil;* and we think it wrong to postulate such fundamental differences in genesis when observation reveals only continuous gradation. Therefore, either all the observed rocks passed through the liquid state or they remained in the solid state. Nor have we the right, in our opinion, to imagine a semimelted state between the two.

Before closing, we should like to point out that the observations which support our arguments were made by many authors independently in many parts of the world.

As several authors have noted and as Read has emphasized, the pavements, the monuments, and the building fronts of the cities of the world offer countless examples; it seems as though they would shout the fact to petrographers. We would not go so far as to wish, as Read humorously did (1944, p. 50) regarding the twelve columns of Wisdom, that characteristic chips might be detached by air bombardment. But we both have traveled extensively and made the same observations from northern Europe to central Africa and across the United States. As Read has written, "Every city has on free display magnificent examples of granitization and migmatization."

We are even astonished that the phenomenon which is at the base of our conceptions did not attract more attention among geologists. Unquestionably, we are concerned with a very general phenomenon, visible in all the granites of the world, which may therefore be considered characteristic of granite origin. Let those who want to be convinced of it follow Read's advice and look at the "free displays" of their towns.

It is the same with regard to the *double enclave* phenomenon and the digestion of veins. As to diffusion in two directions in the border zones and in the inclusions, we shall only refer to the work of H. G. Backlund and of Bugge on arendalites (1946). We have ourselves pointed out the very general occurrence of intercrystalline corrosion.

Under the circumstances, we really do not see why the importance of the observations should be limited to *some* granites and to distances of but a few feet, especially (as Read pointed out) because it is in the larger granitic systems

that emplacement in the molten state is inconceivable.

We think also that it would be dishonest of us to imagine a different genesis for diorites, gabbros, and other granular rocks. The diorites and gabbros, chiefly the former, often constitute an "endomorphic" alteration of granite. Nor should we forget the digestion of country rock with "endomorphism" of gabbros (Lacroix, 1899), the basification of acid inclusions in gabbros, banded gabbros, and the alternate beds of ariegite and lherzolite.

At the risk of having Niggli accuse us of willingness to revive a "universal theory" incapable of explaining the multiplicity of geologic facts, we shall indicate briefly why we believe the mechanism of diffusion *à double sens* in the solid state has an importance far beyond the case of the so-called "igneous" rocks.

First, as we have already written (1939, p. 145), we think that some "extruded" rocks, i.e., lavas, underwent such diffusions after cooling, with the formation of phenocrysts, which were by no means formed in the first stage of consolidation. Later we observed at St-Jean-du-Doigt in Brittany feldspathization of some of the enclosing rock, leading to the formation of feldspar crystals, which gave these materials an absolutely clear appearance of two times of crystallization (1938*b*).

Observations on other lavas by other authors lead us to the belief that later diffusions have had a very clear influence upon the appearance of these lavas in the solid state.

Approaching, now, the problem of metamorphism in general, such work as Lapadu-Hargues's statistical study demonstrates fractional diffusion (*diffusions fractionées*) in two directions—a process identical with that of granitization. Furthermore, in what is apparently a case of pure dynamic metamorphism, at Tré la Tête in the Mont Blanc system, we showed (1946*b*) by analysis and thin section the existence of these diffusions. This confirmed our idea that there is no break (*coupure*) between regional and dynamic metamorphism.

Lastly, in many gneisses and mica schists, we find the same phenomena of superposition in crystallization between crystals and of crystal corrosion, as in granites.

In the tradition of the French petrographic school and of Auguste Michel-Lévy, we do not think it possible to prove that granitization, contact metamorphism, and general metamorphism are of different natures. Being perhaps less prudent than some other authors (like Bugge, who, however, emphasizes the important role of reactions in the solid state), we do not consider other mechanisms, like liquid infiltration, very important. We would except, of course, the cases of lavas and some metallic vein phenomena. As to Wegmann's intergranular films, they do not seem to have played a very important part.

The following reasons lead us to the above conclusions:

1. Crystal corrosion necessarily implies diffusions in the lattice, and consequently we see no necessity for recourse to another phenomenon.

2. Geological observations seem to show that the larger the crystals, the more intense the exchanges, although the opposite should be true if the major role were played by diffusion along the adjoining surfaces because these surfaces are smaller in proportion as crystal sizes increase.

Lastly, we are more and more convinced of the major role played by diffusion in the solid state in the genesis of

many metalliferous ore deposits, particularly igneous deposits. In 1934 one of us had already expressed this idea as a hypothesis (Perrin). The many discussions now appearing in the literature concerning the nature of some deposits, termed "hypothermal," "contact," or "segregation," and the increasingly frequent observation in ores of replacement structures and of crystallization with corrosion show that their genesis certainly depended upon the same mechanism as that of granular rocks.

In closing this paper, Read's humorous sentence comes to mind (1944, p. 84): "Nobody except him who has learnt less and forgotten less than any Bourbon can deny the validity of the conversion 'of solid rocks to rocks of granitic character without passing through a magmatic stage.' "

We shall only point out to Read that it is not easy to get away from ideas which have impregnated one's mind since youth to such a degree that they have become a habit of thought, a habit of reasoning, a habit of language. Perhaps it is even harder not to be guided by subjective "anthropomorphic" impressions.

As for us, we are convinced that not without an inner qualm do some eminent scientists make use, for the lack of a better one, of the word "convergence."

SUPPLEMENTARY REMARKS

We had finished the preceding article when we received the report of the symposium on the origin of granite held at the December, 1947, meeting of the Geological Society of America. Perusal of this report leads us to add the comments below.

1. Professor Grout, citing Read's discussion of feldspars, writes: "This has no basis in chemistry or in logic." Professor Shand also sees no reason why the same feldspars cannot have their origin in the country rock and in the magma under the same pressure and temperature conditions. Both completely exclude the fundamental influence on crystal growth of the medium in which the crystals were formed. We repeat that there is no probability that crystals identical even to minute details (shape, color, inclusions) could be formed in part by replacement of solid crystals in a solid medium and in part by crystallization starting from a molten magma. In order to be sure, we have discussed the matter with eminent nongeological specialists, and their opinion exactly concurs with ours. Furthermore, we should like to learn how Grout and Shand explain the porphyritic crystals which lie across the boundary of the granite and the country rock, as well as granitic bodies entirely isolated in country rock near the contact.

2. Dr. Bowen criticizes but one citation out of a hundred concerning reactions in the solid state—Greig's work on silica bricks—thinking, perhaps, that this one would throw doubt on our entire theory. He makes no reference to Adams' observations or to Perrin's experiments on refractories, in the course of which not the slightest trace of liquid could be found. Our experience with metallurgical linings enables us to state that if we mentioned a "purely hypothetical intermediary liquid" (*intermédiaire liquide purement hypothétique*) to explain the reaction mechanism, it was because we thought that, in the case of such phenomena, the existence of liquids is not the cause but the result; diffusion of elements in the solid state lowers the melting points of refractories while altering their composition, and liquid parts are formed.

Bowen cites, as evidence against our theory, the existence of zoned feldspars,

apparently claiming that the speed of homogenization of heterogeneous crystals of the same crystalline system are, in principle, the same as all rates of ionic diffusion, particularly those which lead to the replacement by substitution of one crystalline network for another. This is certainly erroneous (Perrin and Roubault, 1937, p. 58). Homogenization is a phenomenon which in many cases requires high temperatures. Bowen (1926) has shown that for the solid solution of NH_4Cl–NH_4NO_3 the homogenization temperature is very near the melting point. In our opinion it is much harder to understand why homogenization did not take place at the time of the certainly very slow cooling of a previously molten magma than is the case of reactions in the solid state, which experiments have shown to take place at temperatures much lower than the melting point (for example, BaO and SiO_2 at 357° [Hedwall] CaO+sillimanite, resulting in metasilicate of lime and alumina at 530° [Taylor], etc.).

Why did homogenization not take place in the process considered by Bowen, which requires much higher temperatures than ours? Everyone knows that speed of diffusion increases very rapidly as the temperature increases. Bowen would give much better support to his theories if he would indicate how they can account for plagioclase with completely reversed zoning: labradorite in the center and anorthite around it. How can they explain the existence of feldspars in diorites, theoretically magmatic in origin but which are, in fact, one of the endomorphic facies of granite (Lacroix, 1898–1900)? This phenomenon of inverse zoning is in direct contradiction with Bowen's own experimental diagram of solidification.

In a different way, Bowen criticizes the theories of Ramberg and Bugge, which are merely the first tentative efforts at the erection of a theoretical basis for diffusion in the solid state as applied to geological problems.

We believe the logical way to make progress in this problem must be (*a*) the making of field observations, (*b*) the drawing of scientific deductions from these observations, disregarding "anthropomorphism," and (*c*) the construction of a general theory of the processes.

It is the general aspect, as seen in the field, which has led us to believe in the process of diffusion in the solid state.

It is almost inconceivable that Ramberg and Bugge could have succeeded, at the very first attempt, in formulating theories which could explain everything in a practically unexplored field like that of large-scale ionic diffusion. These authors, nevertheless, deserve a great deal of credit, as does Bowen for his attempt to set up a scientific magmatic theory. With a few exceptions, we might have agreed with him, if accurate observation had not shown that the facts demanded concepts of an entirely different nature.

3. Professor Read, although he favors a large part of our theories (since 1942 he has accepted unaware practically all our essential arguments concerning feldspars in the country rock), calls us "enthusiasts." He considers that we go too far and that there are "granites and granites," although he seems to leave but a small place to magmatic granites.

We objected to the theory of total convergence of structure between rocks which would originate from entirely different genetic processes; we will apply to geology a principle applied in other fields, that is not to introduce an artificial hiatus where none has been observed.

We note that in the course of the very

interesting discussion at the Ottawa meeting, many petrographers asked for definite criteria for distinguishing the two kinds of granites, as there seemed to be no such criteria. This clearly shows that no one knows where the "hiatus" should be placed, and it is an encouragement for us to remain "enthusiasts." The only real, visible distinction, as indicated by Dr. Chapman, is the nonintrusive character at depth and the intrusive at higher levels. But it so happens that the same body of granite has the two aspects, depending on the part of the border under consideration. Besides, feldspathization of country rock is considered clearer and more conclusive evidence of the intrusive origin.

These remarks justify the special importance which we attach to evidence derived from accurately obtained samples in deciding that the intrusive aspect is not evidence of liquid flow but that, on the contrary, it coexisted with unquestionable cases of replacement in the solid state. We are pleased to note Goodspeed's striking example of granodiorite dikes with zoned feldspars, formed by replacement, and Nelson's similar observations. The simple anthropomorphic impression of injection of a liquid can no longer be taken as proof of such injection.

Participants in the symposium discussed the percentage of granites and granodiorites originating from magmas or by replacement in the solid state (despite convincing criteria for distinguishing them), and 15 per cent seems to have been suggested as a minimum for the latter by less optimistic members. In 1934 and 1935 Perrin came to the conclusion that metamorphism took place by diffusion in the solid state and that granites were formed as a result of this process. He would have been called an enthusiast if he had estimated that 15 per cent, or even 1 per cent, of granites were thus formed.

There is, therefore, some progress in our direction, and the recent discussions at the International Geological Congress in London have shown that we are not the only ones who think there are no magmatic granites in the strict meaning of the word. We admit, of course, that granites may result from the granitization of a lava, particularly of a rhyolite.

Professor Read states that he has yet to be convinced of the possibility of the abrupt stopping of metamorphism by diffusion on meeting porous layers. We do not see how he, as a "metamorphist," can otherwise explain the genesis of certain granites with strata of essentially unaltered quartzite within or in contact with the granites (Barrois, Lamotte and Routhier, Hietanen, *et al.*). The septa of limestone described by Moehlmann at the Ottawa meeting and many other phenomena also support this idea.

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VILLAFRANCHIAN STRATIGRAPHY IN SOUTHERN AND SOUTHWESTERN EUROPE[1]

HALLAM L. MOVIUS, JR.
Peabody Museum of Archaeology and Ethnology, Harvard University

ABSTRACT

The deposits containing the classic Villafranchian fauna in Italy (Po Valley, upper and lower Arno Valley, and elsewhere in the peninsula), the Saône Valley of eastern France, and the Puy-en-Velay Basin (Haute-Loire) of central France not only consistently overlie sediments laid down during Upper Pliocene times but also are, in turn, immediately overlain by deposits that clearly belong to the first interglacial (Cromer Forest-Bed–Saint Prestian) stage. With the exception of peninsular Italy, where a marine regime still prevailed, the beds in question, of continental facies and belonging to a Villafranchian stage, represent a comparatively sudden break in the long sedimentary cycle that had prevailed in southern and southwestern Europe throughout Middle and Upper Pliocene (Plaisancian-Astian) times. During the Villafranchian stage, a marked lowering of the temperature occurred, supporting the view that the deposits laid down at this time were accumulated under conditions of glaciation in northern Europe, the Alps, and the Pyrenees. Furthermore, immediately prior to this phase of climatic change, a series of orogenic movements, apparently connected with a time of marine regression, profoundly modified the entire Apennine region. On the basis of these facts, the Villafranchian stage should not be retained in the Pliocene but should be considered basal Pleistocene.

INTRODUCTION

It is becoming increasingly apparent that no real progress can be made with regard to establishing a valid chronology as the basis for comparing the earliest finds of fossil man and of paleolithic implements in Europe and Asia, on the one hand, and the continent of Africa, on the other, until the status of the European deposits containing the well-known Villafranchian fauna is determined. It is generally admitted that in most of the Northern Hemisphere, at least, the beginning of the Pleistocene should be placed at the major climatic change which resulted in glaciation on a worldwide scale. Pilgrim (1944, p. 28), Zeuner (1945, p. 175; 1946, pp. 182–183), Flint (1947, p. 208; 1948, p. 546), and Cooke (1948, p. 43), the most recent authorities to discuss this problem, all seem to agree on this point. As regards the deposits themselves, however, there is as yet no widely accepted interpretation concerning what constitutes the actual stratigraphic base of the Pleistocene (i.e., what beds were laid down during the time of the earliest glacial stage) or, conversely, when the Pliocene came to an end.[2] In so far as Europe is concerned, these fundamental questions have not been adequately considered in the light of the sum total of the available field evidence since 1911, when Émile Haug published his *Traité de Géologie*.

At first glance it appears as though Haug's now-famous definition of the Pleistocene (Haug, 1911, p. 1767) were based entirely on paleontological evidence. However, careful analysis of his argument reveals that it was primarily on the basis of his extremely thoroughgoing and objective examination of all the available facts which led Haug to conclude that the climatic and geologic base of the Pleistocene coincided with certain paleontological data and that this hori-

[1] Manuscript received January 28, 1949.

[2] For discussion and review of the status of the Pliocene-Pleistocene boundary problem in the light of Lyell's original definition and later work by Depéret, De Stefani, De Lapparent, Gignoux, and others see Flint, 1947, pp. 197–200; Schultz and Stout, 1948, pp. 571–574.

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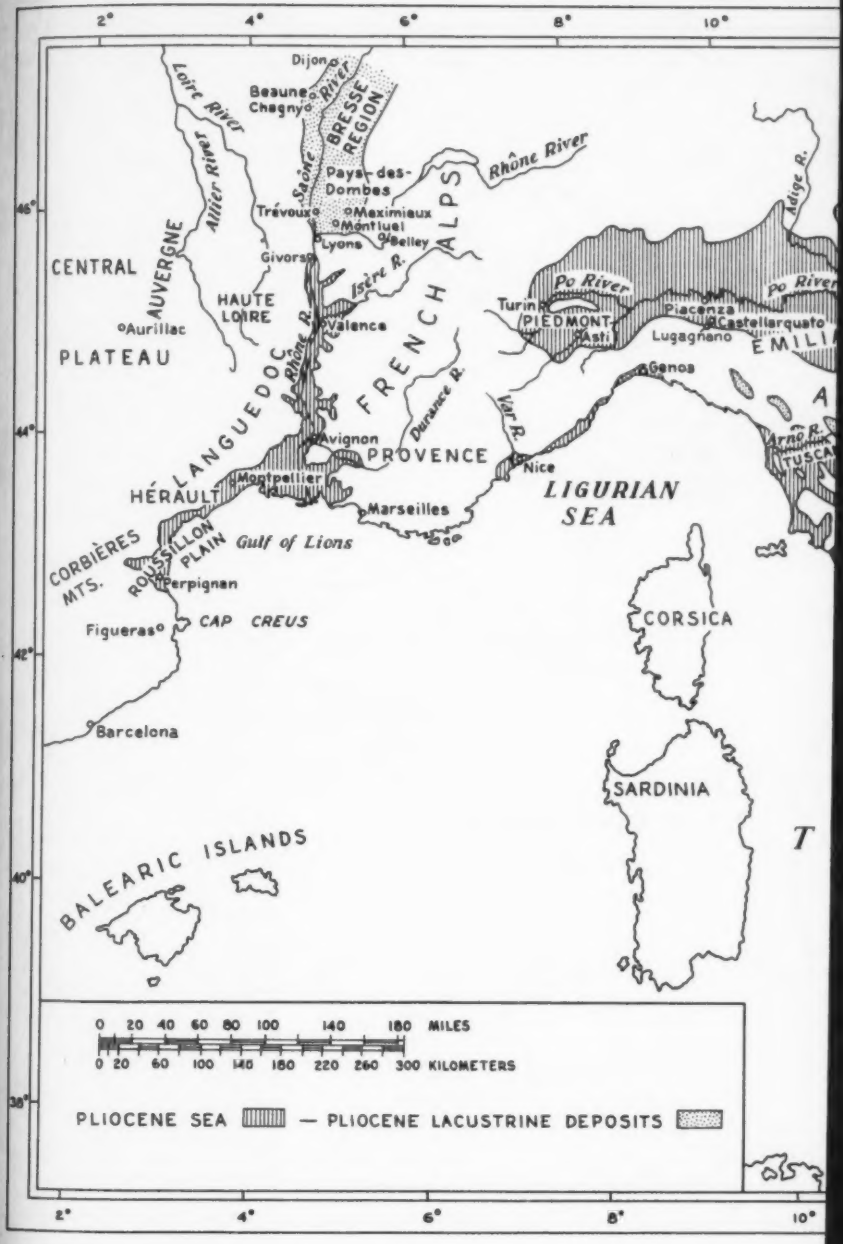
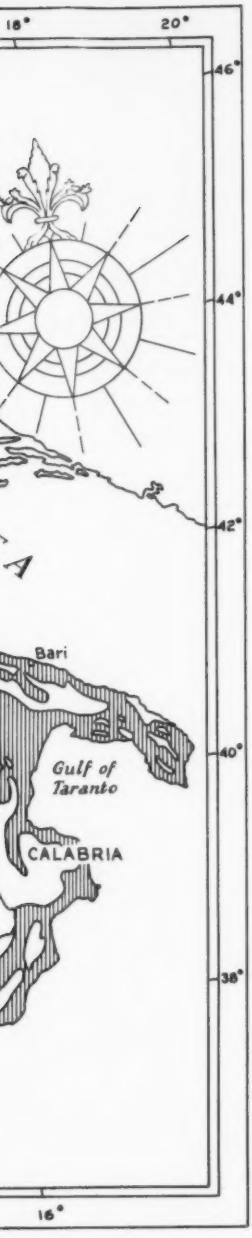


FIG. 1.—Paleogeographic map of southern and southwestern Europe during Pliocene time, showing
dinia, and northeastern Spain are not shown. (Based on Fontannes, 1879–1882; Delafond and Depé

Pliocene time, showing the location of sites mentioned in the text. The Pliocene marine depo
; Delafond and Depéret, 1893; Leonardi, 1940; and Gignoux, 1943, fig. 133.)



marine deposits of Corsica, Sar-

FIG. 2.—Map of southern and southwestern Europe, showing the location of Villa
Boule, 1892; Delafond and Depéret, 1893; Gignoux, 1913, pl. 5; and Merla, 1940.)

owing the location of Villafranchian and later localities mentioned in the text. (Based on Gaudin and Strozzi, 1859; Sacco, 1890; 5; and Merla, 1940.)

zon was marked by the comparatively sudden introduction into Europe of the so-called "Villafranchian" fauna. Therefore, it is not the actual appearance of this fauna which Haug believed to be of fundamental importance; rather it is the fact that this fauna happens to occur at many localities in Europe in beds which this eminent authority was convinced were laid down during the first glacial stage of the Alps, Pyrenees, and Scandinavian regions. The evidence from Italy, Sicily, the Saône Basin of eastern France, and the Puy-en-Velay Basin of Haute-Loire to be considered in this paper[3] supports Haug's conclusion. It demonstrates not only that the Villafranchian represents a clearly defined stratigraphic unit overlying definitely Pliocene beds and is, in turn, overlain by deposits admittedly of the first interglacial stage, but also that, collectively, these deposits belong to a new cycle of sedimentation, probably reflecting the impact of climatic conditions colder and wetter than those now prevailing in southern and southwestern Europe.

THE PO AND ARNO VALLEYS AND OTHER LOCALITIES IN THE ITALIAN PENINSULA AND SICILY

The sequence of the various Pliocene and Pleistocene marine stages of the Mediterranean has been worked out in greater detail in Italy than anywhere else in southern Europe. Throughout this region the Pliocene is abundantly represented by thick marine deposits: clays accumulated in relatively deep water and sands laid down in coastal waters (fig. 1). Because the clay series, collectively known as the Plaisancian, is normally overlain by a sandy facies, grouped under the general term Astian, these formations have been regarded as representing two successive stages in the late Cenozoic succession of Italy. Investigations during the last few years, however, have clearly shown that this is not necessarily the case and that the marine clays and sands in question are merely different facies of the same stage (Gignoux, 1943, p. 528; Migliorini, unpub. MS). In the present paper, therefore, the two formations are referred to collectively as the "Plaisancian-Astian," although at many of the classic late Cenozoic localities in the Italian peninsula these deposits are normally superposed.

Of fundamental importance with regard to Villafranchian stratigraphy are the data which have been observed and studied at numerous exposures in the Po and Arno valleys and at several localities in southern Italy and Sicily. A summary of these observations and their stratigraphic significance follows.

PO VALLEY

Based on the detailed studies of Sacco, Pareto, De Stefani, Gignoux, and others in the southern portion of the Po Valley, the succession of the various late Cenozoic strata near Castellarquato (fig. 3) have become classic (cf. De Stefani, 1891, p. 326; Gignoux, 1915, pp. 338–345; 1916, pp. 26–35; 1923; 1943, pp. 550–552; Cushman, 1945). Here the deep-water blue clays of Plaisancian facies[4] are

[3] Consideration of three important localities in the Allier Valley: Perrier, near Issoire; Mont Coupet, near Paulhaguet, and some 20 km. southeast of Brioude; and Senèze, located midway between Brioude and Paulhaguet, is omitted in this paper. Because the age of the deposits at Mont Coupet and Senèze can be established only on the basis of the interesting paleontological material found at these localities, the evidence of the deposits will obviously throw no light on the problem of Villafranchian stratigraphy. With regard to Perrier, lack of space prevents a discussion of the geological situation in the Issoire region, which is extremely complex.

[4] Plaisance, the type locality for this facies of the Pliocene in Italy, is a small village on the road

conformably overlain by the littoral or shallow-water yellow sands of the Astian;[5] the latter is, in turn, surmounted by the littoral sands with *Cyprina islandica* L., of the Calabrian. In 1865 Pareto[6] proposed Villafranca d'Asti as the type locality for the Villafranchian stage, and since then this term has been widely used with reference to those beds in southern and western Europe (as well as elsewhere in the Old World), which, on the basis of their stratigraphic position and contained fauna, have been assigned to this portion of late Cenozoic time. Most of the deposits of this stage consist of fluvial or fluvio-lacustrine sediments (fig. 2); at numerous localities these contain terrestrial and fresh-water fossils, including land snails. All along the northern Apennine slopes in the southwestern portion of the Po Valley the essentially continental Villafranchian sediments immediately overlie the marine clays and sands of the Plaisancian-Astian stage of the Pliocene epoch.[7]

Although in the region of Asti the Villafranchian cycle of deposition is superposed directly on Pliocene sands, much of the rest of Italy, with the exception of the highland areas, was submerged. For the marine equivalent of the continental Villafranchian facies the term Calabrian, from the Province of Calabria in southern Italy, where deposits of this stage are extensive, was introduced in 1910 by Gignoux.[8] The invertebrate marine fauna of the Calabrian, studied in detail by Gignoux (1913, pp. 29–176), is interesting. In ad-

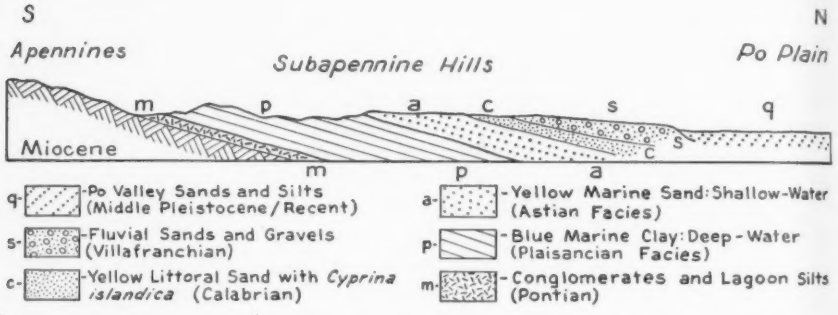


FIG. 3.—Schematic section showing the Pliocene and Pleistocene deposits in the vicinity of Plaisance, near Castellarquato, in the southern portion of the Po Valley. (After Gignoux, 1943, fig. 132.)

between Castellarquato and Lugagnano. The term "Piacenzian," proposed by Gignoux in 1923, has never come into general use.

[5] Named after the interesting exposures in the region of Asti, Piedmont.

[6] Pareto, 1865, pp. 262–270. Villafranca d'Asti is a small town located on a tributary of the Tanaro River, some 15 km. due west-northwest of Asti; it lies on the railway line that runs between Asti and Turin.

[7] Depéret (1885*a*, p. 248; 1893, p. 528) points out the interesting fact that the Upper Pliocene deposits of southern France contrast markedly with those of northern Italy; for the evidence from both Montpellier and Roussillon (Perpignan), in the former region, demonstrates that a regime of fresh-water deposition was already well established by Upper Pliocene times. In Italy, however, this general movement was later, as proved by the sequence in the Asti region. After terrestrial conditions had become established in the Po Valley, the emergence of the Italian peninsula apparently progressed in a general north-to-south direction. Thus, in the south of Italy, marine sediments were being laid down contemporaneously with continental deposits in the north.

[8] 1910, p. 841; 1911, p. 115; 1913, p. 22. For the more or less transitional zone between the Calabrian

dition to an overwhelming majority of living species (over 95 per cent),[9] the Mediterranean was invaded during the time represented by these beds by a number of "cold" boreal types of mollusks from the North Atlantic. The new species, which suddenly appear here for the first time in Italy in Calabrian deposits, include the following:[10] *Cyprina islandica*, *Buccinum undatum*, *B. humphreysianum*, *Natica montacuti*, and *Trophon muricatus*. Although no studies have ever been made regarding the frequency of the occurrence of these forms with respect to the total assemblage of the marine fauna from any one of the main Calabrian localities, their appearance in the Mediterranean at this time is generally regarded as demonstrating markedly cooler marine conditions than those that had prevailed during Pliocene times.

This entire complex, comprising the continental Villafranchian and the marine Calabrian horizons, is included in the Pleistocene by Haug (1911, p. 1860), Rovereto (1925, p. 209), Blanc (1942*a*, p. 154), and Di Napoli-Alliata (1947, pp. 19 and 24). On the other hand, because Gignoux came across no exposures during the course of his extensive field work in Italy and Sicily that demonstrated an apparent sedimentary break between the Plaisancian-Astian and the Calabrian, he placed both these formations in the same sedimentary cycle. On this basis, Gignoux concluded that the Plio-Pleistocene boundary in Italy should be placed between the Calabrian and the next younger formation, the Sicilian. The results of recent investigations of this problem, however, have completely invalidated Gignoux's argument by demonstrating that the boundary in question coincides with the first appearance of northern immigrants in the faunistic succession of Italy, an event which occurred at the base of the Calabrian.

In the last few years several sections, composed in the main of marine deposits, have been recorded during the course of well-boring operations on the Po Plain (cf. Borghi, 1941; Di Napoli-Alliata, 1946*a*). According to Di Napoli-Alliata (1946*b*, p. 29), a series of deposits thus exposed near Rovigo clearly show the transgressive nature of the Calabrian. On the basis of the facts that (*a*) the basal Calabrian deposits here occur as a littoral formation, (*b*) the middle zone of the same horizon was laid down in relatively deep water, and, (*c*) finally, a series of littoral deposits demonstrate a new shallowing of the sea, Di Napoli-Alliata concludes that the Calabrian forms a complete sedimentary cycle. Furthermore, there is a most striking difference between the microfauna in the lowermost Calabrian deposits as contrasted with the Plaisancian-Astian beds (Di Napoli-Alliata, 1946*b*, p. 28). Approximately 10

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and the Villafranchian, characterized by brackish-water or lagoonal deposits, Sacco (1886) proposed the name "Fossanian"; but, because no characteristic fauna has ever been found for this zone, the term Fossanian is no longer used (see Gignoux, 1911, p. 120; 1913, p. 316). Apparently it represents the very base of the Villafranchian. (Cf. De Stefani, 1891, p. 322; Depéret, Mayet, and Roman, 1923, pp. 43–44.)

[9] As Gignoux points out (1922, p. 1453), the Calabrian deposits actually contain very few extinct species; those once thought to be extinct have in reality migrated to other regions, where they are still living.

[10] Gignoux, 1911, p. 117; 1913, p. 599; 1922, p. 1453. The whole question of the value of certain types of mollusks as indicators of late Cenozoic climatic conditions in the Mediterranean region is in urgent need of further investigation. In this connection, Dalloni (1940, p. 13, n. 3) reports the presence of *Cyprina islandica* L. in deep-water clays of Plaisancian facies on the Algerian coast, and Zeuner (1945, p. 232) records the same so-called "North Atlantic" form living at present in the Bay of Biscay as far south as the Bordeaux region.

per cent of the entire Calabrian assemblage is composed of cold forms, which thrive today in the North Atlantic, and these suddenly appear at the base of the formation. These are completely absent in the Plaisancian-Astian series. The significant quantitative differences revealed by this fauna have been summarized by Migliorini in a paper to be published in the forthcoming *Proceedings of the 18th International Geological Congress*, held in London in 1948.

From a second section near Lodi, southeast of Milan, Di Napoli-Alliata (1947, pp. 20–21) has described the plant remains found in a peat layer which is intercalated in a series of Calabrian clays and sands. Not only does this flora indicate that cool temperate conditions prevailed at the time of deposition, but also the Foraminifera from the same locality agree in every respect with this conclusion. As regards the flora, Di Napoli-Alliata states that, in place of the tropical to subtropical plants and trees that generally characterize the Pliocene deposits of this region, an essentially north-temperate assemblage was found, including *Alnus*, *Pinus*, *Abies*, *Castenea*, Ericaceae (a northern European family of heather), and various types of ferns. This flora indicates a climate somewhat cooler and wetter than that which prevails in northern Italy today. The subsurface deposits at the Lodi site also provide further evidence for a sudden shallowing of the sea at the base of the Calabrian as far west as the Milan region of the Po Plain.

South of Modena in the Apennine foothills fringing the southern edge of the Po Plain the transgressive nature of the Calabrian has also been observed at a newly excavated section near the town of Sassuolo. Here Montanaro-Gallitelli (1947, pp. 6–9) has described a clearly defined unconformity separating the Pliocene marine clays (Plaisancian facies), and the sands and gravels of the Calabrian series. This evidence confirms Ruggieri's conclusions based on his studies in the Santerno Valley, near Imola, some 70 km. southeast of Modena (Ruggieri, 1938, 1939, 1944). Thus, at a number of localities in the Po Valley the Calabrian beds have been recognized as being stratigraphically independent of the underlying Pliocene deposits, and there is a substantial amount of evidence demonstrating that the new cycle of sedimentation coincides with an abrupt break in the climate. But it is possible that the Calabrian itself covers more than one stage. Recent stratigraphic studies by Selli and Ruggieri in Emilia (Po Plain) favor the view that a new unit, Calabrian II or Emilian, with a mild-climate fauna, should be recognized between the Calabrian (*sensu stricto*) and the Sicilian (unpublished: paper presented at the 18th Internat. Geol. Cong., London, 1948; cf. Oakley, 1949*a*, *b*). However, the correlation between this new unit and the long interval of emergence accompanied by tectonic movements and erosion, which separates the Calabrian clays from the Sicilian yellow sands in the Imola region (Ruggieri, 1944, pp. 109–110), has not as yet been established.

Gignoux's detailed work on the Calabrian deposits of the southern portion of the Po Valley and along the east coast of Italy (Gignoux, 1915, 1916, 1922, p. 1485; 1923, pp. 374–375; also De Stefani, 1891, p. 322) has demonstrated that the replacement of this ancient northern Italian gulf by continental conditions progressed in a general west-to-east direction. Thus at Villafranca and numerous other localities in the Asti region, fresh-water deposition was already under way at the beginning of the Calabrian cycle of sedimentation. These latter de-

posits, which are mainly of fluvial origin, have yielded rich collections of the classic Villafranchian mammalian fauna.[11] Although the latter occurs throughout the widespread sands and gravels of this stage, which are preserved in the Asti region, the basal portion of these continental sediments is especially prolific of fossil bones. In fact, Depéret (1893, p. 531) states that at several localities proboscidian (*Archidiskodon planifrons* Falc.) molars with oyster shells attached to their surface have been collected (cf. De Stefani, 1891; also above, n. 8). However, with the exception of this single observation, one can learn little else concerning the nature of the actual gravel and sand beds which yield fossil vertebrate remains in this key area. Because much of this material was found in the last century when the excavation was being made for the bed of the railway between Asti and San Paolo di Villafranca, the stratigraphy should doubtless be rechecked by up-to-date methods. In any case, there appears to be no valid basis for the claim that bones and molars of true *Elephas* occur in this region in beds that are demonstrably of Pliocene age. The fact that in the vicinity of Imola abundant remains of a Villafranchian vertebrate fauna have been found in a sandy littoral deposit yielding mollusks typical of the Calabrian (Gignoux, 1915, p. 334; 1916, p. 22; 1923, p. 374; Ruggieri, 1938, 1939, 1944), on the other hand, provides further convincing proof that the Calabrian is definitely the marine counterpart of the essentially continental Villafranchian.

BASIN OF THE UPPER ARNO RIVER

During the last hundred and fifty years, enormous collections of fossil vertebrates have been amassed from various localities in the basin of the upper Arno River, where the late Cenozoic freshwater sediments of this portion of Tuscany are well exposed. In point of fact, it is these deposits in the upper Val d'Arno which have furnished the type vertebrate fauna for the Villafranchian stage. This basin is filled with material that is entirely of fluvial and lacustrine origin (fig. 4) and which attains a thickness of over 350 meters. Its surface forms a broad, flat plain between Montevarchi, near Arezzo, on the south and Rignano-in-Valdarno in the north—a total distance of over 70 km., while to the south of Arezzo a long arm of this basin extends up the Val di Chiana to the vicinity of Chiusi, some 15 km. southwest of Castiglione (Merla, 1940; Losacco, 1944). There are two main series of deposits represented by the Val d'Arno Basin sediments, as follows:[12]

Lower series: Tilted beds of fine gray or blue lacustrine clays and silts (known to Italian geologists as *stellicione*), which contain fresh-water mollusks, layers of lignite, and a poorly represented mammalian fauna.[13] This horizon is of Plaisancian-Astian age.

[11] For lists see Depéret, 1885*a*, pp. 256–257; 1893, pp. 530–531; Haug, 1911, p. 1861; Depéret, Mayet, and Roman, 1923, pp. 44–45.

[12] The writer is profoundly grateful to Professor Giovanni Merla, of Florence, for his generosity in providing an exceptionally clear statement of the results of his recent and as yet unpublished investigations in the Val d'Arno superiore. See also Gaudin and Strozzi, 1858–1859; Ristori, 1886*b*; Gignoux, 1913, pp. 304–309; Depéret, Mayet, and Roman, 1923, pp. 45–52; Sestini, 1936, pp. 38–39; Migliorini, unpublished paper to appear in the forthcoming *Proceedings of the 18th Internat. Geol. Cong.*

[13] The mammalian fauna from the *stellicione*, which includes *Anancus arvernensis*, *Sus provincialis*, *Tapirus* sp., and *Machairodus* sp., constitutes an assemblage without any well-determined character (cf. Depéret, 1893, p. 532). Apparently, this is the assemblage which Schlesinger (1922, p. 216) assigned to his "early" Val d'Arno horizon, his "late" horizon being the classic group mainly from the *sansino*, in which *Equus stenonis* Cocchi, *Leptobos etruscus* Falc., *Archidiskodon meridionalis* Nesti appear. Although the former is admittedly of Plaisancian-Astian age, Schlesinger's conclusions

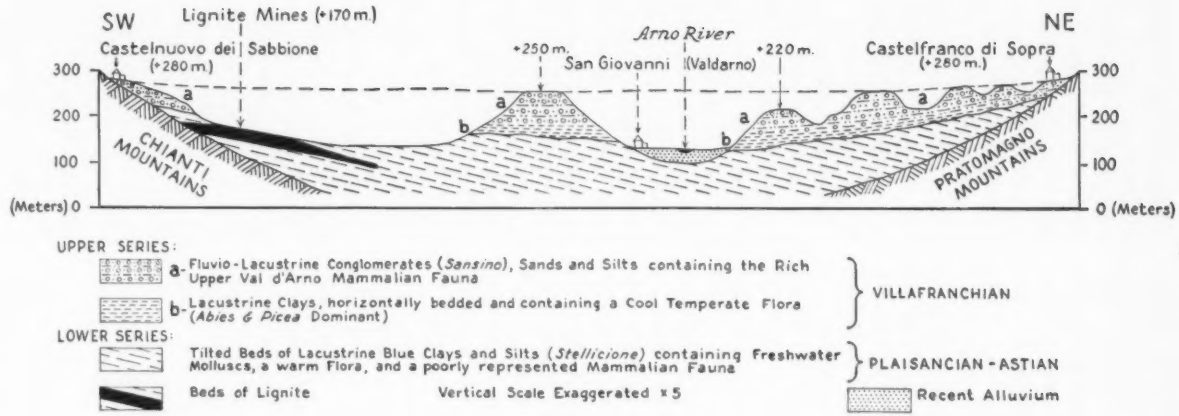

FIG. 4.—Geologic section through the basin of the upper Val d'Arno, Tuscany. (After Gignoux, 1913, pl. 3, fig. 3; modifications based on Sestini, 1936, and Merla, unpublished ms.)

Upper series: These beds are horizontal throughout and are separated from those of the lower series by an unconformity. They may be subdivided into two clearly defined zones:

Zone b—Lacustrine clays and silts, very similar to those of the lower series, comprise the basal member of the upper series. These clays are rich in pollen of trees which thrive under cool temperate conditions.

Zone a—The fluvio-lacustrine gravels, or conglomerates (*sansino*[14]), micaceous sands and silts that occur throughout this zone, are of a ferruginous color. At many localities where these beds have been dissected and exposed by erosion, they are extremely rich in the remains of a typically Villafranchian vertebrate fauna.[15]

The character of the deposits of the upper series demonstrates that during Villafranchian times the late Cenozoic lake basin of the upper Val d'Arno was being transformed into a vast alluvial plain. According to Dr. Merla, all the polleniferous beds of Zone b occur below the level of the lowest strata in which the Villafranchian mammal remains have been collected. Because the beds of the upper series are practically horizontal throughout and because there is no evidence of an erosion interval during the time represented by the deposition of Zones b and a, it may be concluded that the mammal-bearing levels are everywhere more recent than those containing pollen. In any case this vertebrate fauna comes in the main from the uppermost 100 meters or less of the upper series—i.e., from Zone a and the top of the clays of Zone b.

It was on the basis of the evidence from the upper Val d'Arno that the Italian geologist, Sestini (1936), first demonstrated the fallacy of the concept of a stratigraphic continuity between the Villafranchian and the underlying definitely Pliocene beds. Sestini's work verifies the occurrence of pre-Villafranchian tectonic movements in this section of Tuscany and proves that the Villafranchian beds of the upper Arno Basin are separated from the older Plaisancian-Astian terrain by an unconformity. According to Sestini (1936, pp. 38–39), the strata comprising the upper (Zones a and b) horizon are nearly horizontal throughout, whereas in the lower (*stellicione*) series the bedding dips smoothly toward the north or northeast. In places the angle of tilt approaches 90°, but in the main it is not over 15°. A stratigraphic hiatus also occurs in the region immediately to the south; in the Val di Chiana, Losacco (1944, p. 65) has demonstrated that the Plaisancian-Astian beds are unconformably overlain by a thick series of lacustrine sands, clays, and gravels that contain a typically Villafranchian mammalian assemblage. The rich flora which has been documented from the layers of lignite in the upper part of the Plaisancian-Astian deposits of the upper Arno Valley is well known on the basis of the classic studies by Gaudin and Strozzi (1858–1859) and later by Ristori (1886), and it shows that tropical to subtropical conditions prevailed here during Pliocene times, just as elsewhere in southern and western Europe (cf. Sismonda, 1864,

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have been widely quoted as the authority for subdividing the Villafranchian stage as a whole into an "early" and a "late" horizon (cf. Zeuner, 1945, pp. 257–258; Schreuder, 1945, p. 190). Manifestly, no such subdivision is justifiable on the basis of the available data.

[14] *Sansino* is an Italian word that comes from *sansa*, the residue of crushed olives; that is to say, gravels containing elements up to the size of an olive (see Depéret, Mayet, and Roman, 1923, note on p. 46). The term is of no particular stratigraphic significance.

[15] For lists of the upper Val d'Arno fauna see: Depéret, 1885*a*, pp. 257–258; 1893, pp. 532–533; De Stefani, 1891, pp. 317–318; Forsyth Major, 1875, 1876, p. 345; 1885; Kormos, 1932; Depéret, Mayet, and Roman, 1923, pp. 50–53; Osborn, 1942, pp. 969–970; Schaub, 1928, 1943, pp. 286–287; Stehlin, 1934; Weithofer, 1889, 1891.

pp. 470–471; De Saporta, 1879, pp. 345–346; and Depape, 1928, pp, 47 and 67). Typical forms include *Sequoia*, *Taxodium*, *Liquidambar*, *Cinnamonum*, *Magnolia*, and *Cassia*, as well as other genera that thrive today in a warm climate. These forms contrast markedly with those represented in the spectra recently obtained by Dr. G. Merla (unpublished) from the peat beds intercalated in the Villafranchian lake sediments of Zone b of the upper series of this basin. The preliminary analysis of this pollen indicates the sudden spreading and rise to dominance of *Abies* (Fir) and *Picea* (Spruce) in this stratum, together with a rather high percentage of *Pinus* (typus *silvestris*) and substantial amounts of *Fagus*, *Alnus*, and *Tilia*. This assemblage provides convincing proof that the climate of the northern Apennines was perceptibly cooler than that of the present at the time these beds were laid down. Therefore, not only is the Plaisancian-Astian series of the upper Val d'Arno Basin stratigraphically and paleontologically quite distinct from the Villafranchian, but also the floras from these two deposits reveal that a marked climatic break occurred, which is suggestive of the onset of a glacial stage in the regions to the north.

SITES IN THE LOWER ARNO VALLEY

As stated by De Stefani in 1891,[16] the late Cenozoic marine deposits of the lower Arno Valley, which belong to the Calabrian stage and which are overlain by fresh-water sediments, are closely parallel to and contemporaneous with the lacustrine deposits of the upper part of the same valley. This stratigraphic continuity between the fresh-water sediments of the upper Val d'Arno and the dominantly marine beds which extend from Empoli to the sea in the lower Val d'Arno agrees in all essential respects with the evidence summarized above from the Po Valley north of the Apennines (cf. Gignoux, 1916, p. 34); for, in that portion of Tuscany lying between Empoli and Pisa, the deep-water marine blue clays and silts of Plaisancian facies are overlain by a thick series of coarse yellow sands intercalated with gravel layers (fig. 5). Although no recent work has been done in this region, the basal part of the latter horizon is considered to belong to the Astian series, whereas the upper levels have been assigned to the

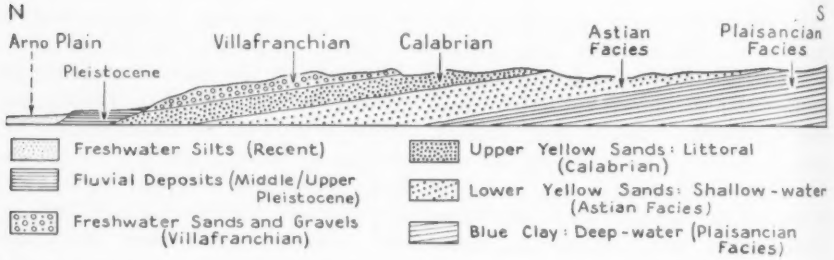


FIG. 5.—Diagrammatic section showing the sequence of late Cenozoic deposits in the lower Arno Valley, south of Pisa. (After Gignoux, 1913, pl. 3, fig. 2).

[16] De Stefani, 1891, pp. 314–317; see also Forsyth Major, 1885, pp. 3–4; Gignoux, 1913, pp. 300–304 and 307–308; 1922, pp. 1464–1465; Depéret, Mayet, and Roman, 1923, pp. 46, 52–53, and 116–118.

Calabrian.[17] Especially in the eastern portion of this region, a considerable thickness of fresh-water silts, sands, and gravels overlies the marine sediments of the Calabrian. The latter mark the final regression of the sea from the valley and in part represent a synchronous prolongation of the uppermost fluvial and fluvio-lacustrine deposits of the upper Val d'Arno.

As is universally the case throughout the latter area, important finds of a typically Villafranchian vertebrate fauna[18] have been made both in the fresh-water gravels and in the upper part of the immediately underlying marine sands. In all its main elements this fauna is identical with that from the upper Val d'Arno, a fact which, together with the geological evidence, provides absolutely no basis for the argument that the mammalian assemblage of the upper Val d'Arno is more recent than that from the lower Val d'Arno. Certainly in no respect whatsoever do the data support Boule's claim for assigning a pre-Villafranchian age to the deposits in the lower Arno Valley containing the *Archidiskodon meridionalis* fauna (Boule, 1893*c*). As far as Italy is concerned, the stratigraphic relationships between the Villafranchian-Calabrian complex and the next younger marine stage, known as the Sicilian,[19] is exceptionally clear. This is especially true in the case of many fine sections in the Leghorn and Anzio regions, as well as in southern Italy and Sicily, where very thick deposits of this stage are exposed.

SOUTHERN ITALY AND SICILY

As demonstrated by the evidence from the upper Val d'Arno, the transgressive nature of the Calabrian on the Po Plain is in accord with the fact that mountain-building processes took place between Plaisancian-Astian and Calabrian times in the northern Apennines. That severe orogenic movements did, in fact, occur at this time throughout the entire Apennine chain is proved by Migliorini's recent observations in the Candela-Melfi region, south of Foggia. Here, in the Apennine foothills of Puglia, the Plaisancian-Astian series is bent into recumbent folds, whereas just to the east there occur Villafranchian beds which are practically horizontal. Furthermore, near Sala Consilina, some 76 miles due southeast of Naples, the Apennine divide is capped by a marine Plaisancian-Astian outlier at an elevation of some 1,150 meters above sea-level (Migliorini, 1946). At the base of the scarps on either side of this watershed there is a series of basins, each of which is filled with Villafranchian lacustrine deposits, their surfaces ranging from 650 to 800 meters above sea-level. In spite of this very clear stratigraphic evidence, however, no true unconformity is perceptible between the Plaisancian-Astian and the Calabrian either in the classic region of Calabria (Migliorini, un-

[17] Demonstrated by Gignoux (1913, pp. 303–304) on the basis of his detailed study of the invertebrate marine fauna from several late Cenozoic horizons exposed at Vallebaija and other localities in the vicinity of Leghorn. In this connection, it should be noted that remains of *Archdiskodon meridionalis* have been collected in the Calabrian deposits exposed in the vicinity of Fauglia, near Vallebaija (see Gignoux, 1913, pp. 308 and 614).

[18] For lists see: Depéret, 1893, p. 531; Depéret, Mayet, and Roman, 1923, pp. 53–54 and 118.

[19] For a clear definition of the Sicilian, a stage originally described by Doderlein (1870–1872), see Gignoux, 1908; 1911, pp. 121–123. According to the stratigraphic scheme advocated by Gignoux, the Sicilian marks the base of the Pleistocene. Osborn (1910, pp. 317–321; 1915, p. 222), as well as Boule and Piveteau (1935, pp. 516 and 561), following Doderlein (1870–1872), Seguenza (1880, pp. 315–317), and De Lapparent (1906, p. 1648), group both the Villafranchian-Calabrian *and* Sicilian stages under the general term "Sicilian," which they assign to the Pliocene (cf. Gignoux, 1913, pp. 7–9; Cipolla, 1934*b*).

pub. MS) or in the Messina area of Sicily (Trevisan and Di Napoli-Alliata, 1937, p. 33; Trevisan, 1942, p. 11). It was on this evidence that Gignoux assumed conformity at the many other localities in Italy and Sicily which he studied between 1908 and 1913. Nevertheless, as originally pointed out in the vicinity of Sciacca by Di Stefano (1889, pp. 71–72) and later near Augusta by Maugeri-Patané (1923, pp. 94–95), in the Syracuse region by Trevisan (1936, pp. 10–11), and at several additional localities in southern and southwestern Sicily (Cipolla, 1934*a*; 1934*b*, p. 3; Trevisan and Di Napoli-Alliata, 1937, p. 34; Blanc, 1942*a*, p. 149; Trevisan, 1942, pp. 8–9), there is a very well-defined angular unconformity at the base of the Calabrian which clearly separates it from the underlying Plaisancian-Astian complex. Also, the presence of this well-marked stratigraphic feature at Monte Mario, near Rome (Cerulli-Irelli, 1907, pp. 67–68; Haug, 1911, p. 1645; Blanc, 1942*a*, p. 149), has been established.

At numerous localities in Sicily the transgressive nature of the Calabrian is especially clear, confirming the evidence from the Po Plain, summarized above. From Marsala in southwestern Sicily, all along the south coast, and as far north as Augusta in the eastern portion of the island the Calabrian rests on Pliocene and older rocks, and there is an angular unconformity between this complex and the deposits of the Plaisancian-Astian series. Trevisan (1942, pp. 3 and 8–9) states that during the time immediately preceding the lower Calabrian there was a major phase of uplift followed by an important marine transgression. Although this event is clearly recorded in the Syracuse area, the Messina region forms a special belt in which the discordance between the Pliocene and the Calabrian horizons has not been registered. Because the transgressive portion of the Calabrian series occurs at approximately the level of the present shore, it is concluded that the pre-Calabrian regression caused a somewhat greater extent of the land to be emerged than is the case at present.

The strata exposed at several localities along the western coast of the Italian peninsula—Leghorn region (Gignoux, 1911, p. 126; 1913, pp. 309–313), the coastal plain of Agro Pontino, south of Rome, and between Anzio and Nettuno (Blanc, 1937*a*, p. 629)—clearly reveal the stratigraphic position of the 80–100-meter raised beach deposits of the Sicilian stage in relation to the underlying sediments of the Calabrian-Villafranchian complex (cf. Blanc, 1937*b*, p. 359; 1942*a*, p. 154). In the Anzio area (Blanc, 1937*a*, p. 629) the Calabrian sandstones were "dislocated during the Lower Pleistocene by tectonic movements and partly destroyed by degradation. . . . The post-Calabrian regression was followed by the Sicilian transgression, during which the sandy clays with *Plicatula* and *Brocchia* to the south of Nettuno were laid down in roughly horizontal strata." It is this evidence which serves to connect the late Cenozoic sequence in northern Italy with that recorded at many of the classic localities of the southern portion of the peninsula and Sicily (fig. 6). In each case the Sicilian deposits directly overlie those of the Calabrian, although the two are stratigraphically separated from each other by an unconformity represented by tilting and erosion. In other words, during the interval between the Calabrian and the Sicilian a regression of the sea, accompanied by a new phase of tectonic movements, occurred.

In the Rome region, gravels of Sicilian age are widespread, as pointed out by

Blanc (1948, p. 5). Two localities near the city of Rome have yielded very early lower Paleolithic implements, as follows: (*a*) a heavily rolled hand-axe of Abbevillian type (G. A. Blanc, 1935) and (*b*) two flakes manufactured by the so-called "Clacton" technique (A. C. Blanc, 1936). Although the horizon in question is apparently the same in each case and is believed to date from Sicilian times (cf. Köppel, 1935, p. 475; Blanc, 1942*b*, p. 5; 1948, p. 9), this fact has not as yet been definitely established.

According to Migliorini (unpub. MS), the orogenic movements between the Sicily, e.g., Mortelle, near Messina (fig. 6).

Gignoux's detailed observations in Calabria and Sicily (1908, 1909, 1910, 1911, 1913; summarized by Depéret, 1918, p. 483) demonstrate that the invertebrate marine fauna of the Sicilian stage, the type locality for which is in the region of the Gulf of Palermo, is intimately and directly related to that of the Calabrian deposits of the same area.[20] Recent studies of the mollusks from Sicilian clays at Ficarazzi and Conca di Oro, near Palermo, have further substantiated this conclusion (Tamajo, 1937,

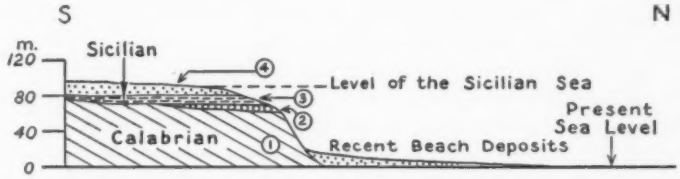


FIG. 6.—Section through the marine deposits in the vicinity of Mortelle, near Messina (Sicily), showing the unconformity between the Sicilian raised beach and the underlying Calabrian strata. *4*, littoral marine sands, very fossiliferous; *3*, beds with *Ostrea edulis; 2*, conglomerates and lagoon silts containing *Cardium edule; 1*, very hard conglomerates with large *Mytilus* shells. (After Gignoux, 1913, fig. 31.)

Calabrian and the Sicilian were very much less severe than those which occurred during the Plaisancian-Astian–Calabrian-Villafranchian interval. In the Apennines very mild mountain-building activity is evidenced by occasional minor tilting and faulting of the Villafranchian lacustrine deposits. Trevisan (1942, pp. 16–17) states that along the coast a vast "Tirrenide" appeared at this time, which was almost immediately transgressed by the sea. But very probably eustatic factors were also involved. In addition to the evidence cited above from the Anzio-Nettuno area, there is a clearly defined unconformity between the Calabrian and the Sicilian in the Palermo and Agrigento regions (Trevisan, 1942, pp. 20–21; Cipolla, 1934, p. 6), as well as elsewhere in p. 466; De Stefani, 1941, p. 280), while the Foraminifera from a new site in southwestern Sicily, where the deposits are carried on an extensive wave-cut terrace that is up to 3 miles wide, actually differ very little from those recorded at Calabrian deposits in the same region (Trevisan and Di Napoli-Alliata, 1937, p. 25). Normally, however, the Sicilian assemblages are characterized by (*a*) the

[20] During Plaisancian-Astian times, a large part of the island of Sicily was under the sea (Gignoux, 1922, p. 1474). Toward the close of the Pliocene, however, deposition was taking place under conditions of gradual uplift, a movement which culminated in the pre-Calabrian emergence (Trevisan, 1942, p. 8). As the result of the subsequent transgression, marine deposits of the Calabrian stage are enormously thick even in central Sicily (Gignoux, 1911, pp. 119 and 125; Trevisan, 1942, pp. 9–16), which is also the case in southern Italy.

disappearance of a few of the "archaic" Pliocene types which persisted in the Calabrian; (*b*) the presence of some, very rare, extinct forms; and (*c*) the occurrence of a limited number of "cold," or boreal, species typical of northern Atlantic waters, most of which had already appeared in Calabrian times. This fauna is found in two types of sediments—true beach accumulations (fig. 6) and ocean-floor deposits—both of which were formed at a time when the sea relative to the land stood some 80–100 meters above its present level (cf. Depéret, 1920, p. 163). As previously stated, these Sicilian marine gravels, sands, and silts rest unconformably on the older Calabrian series. But the erosion interval separating the two horizons must have been of relatively short duration (Trevisan, 1942, p. 22), since, from a paleontological point of view, the dominantly littoral faunas of the Sicilian and Calabrian stages are very closely related. Possibly, further light will be thrown on this problem if deep-sea cores covering this portion of late Cenozoic time can be obtained for analysis. In any case, there is no evidence whatsoever of an uplift during Sicilian times.

The evidence concerning the climate of the Sicilian is confusing. Formerly, on the basis of the "cold" increment in the molluscan faunas of the Sicilian, it was believed that the temperature of the sea during this stage was actually somewhat lower than that of the present Mediterranean. This conclusion is supported by studies of the microfauna from the classic deposits at Conca d'Oro, near Palermo (Di Napoli-Alliata, 1937). On the other hand, most of the so-called "boreal" types, once thought to be new arrivals from the North Atlantic during the Sicilian, apparently were already present as early as Calabrian times. As Gignoux (1922, p. 1475) points out, the forms considered to be indicative of warm conditions are very much more abundant in deposits of this stage than are the typically "cold" assemblages. Furthermore, many localities yield a rich fauna that is neither characteristically "cold" nor characteristically "warm," because the total number of species that serve as critical climatic indicators is very small in comparison with the fauna as a whole. It is hoped, therefore, that some day the data will be available to provide a basis for working out the percentage composition of the molluscan faunas from some of the main southern Italian and Sicilian localities of this stage, just as Davies has done in the case of the Pliocene and Pleistocene deposits of southeastern Britain (cf. fig. 7; Davies, 1934, fig. 28, p. 208; also Boswell, 1936; Zeuner, 1937, 1945, p. 106). With reference to those sections in southern Italy and Sicily at which the so-called "cold" Sicilian fauna has been recorded, Depéret (1918, p. 483) states that doubtless the deposits in question were laid down in certain cold currents at a given depth in the Sicilian Sea. According to this authority, the "cold" forms are completely lacking at many other Sicilian localities in Italy, along the southern coast of France, and in North Africa, where the invertebrate fauna corresponds in all its essential elements with a normal Mediterranean assemblage of the present day.

On the basis of the recent data, Migliorini (unpub. MS) considers it very doubtful that any cooling at all took place in the Sicilian. Indeed, the very meager paleobotanical evidence from near Lodi, southeast of Milan on the Po Plain (see p. 384; Di Napoli-Alliata, 1947, p. 22), strongly suggests that a significant *rise* in temperature may have occurred during early Sicilian times. This

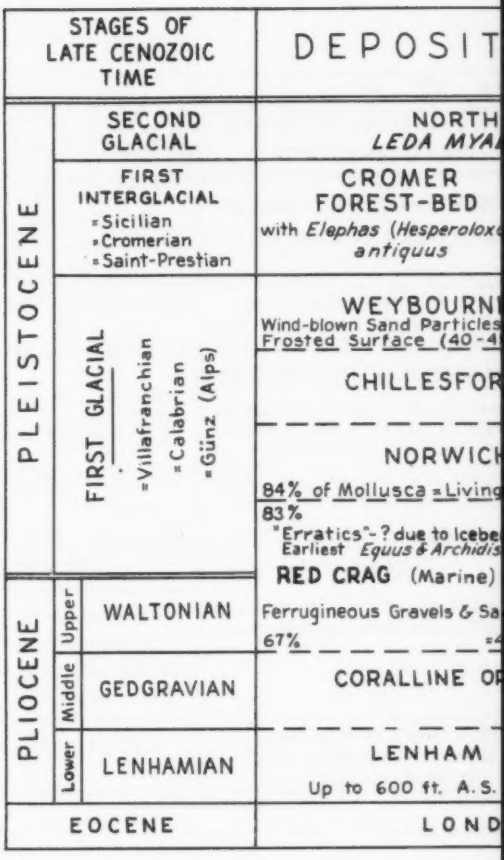
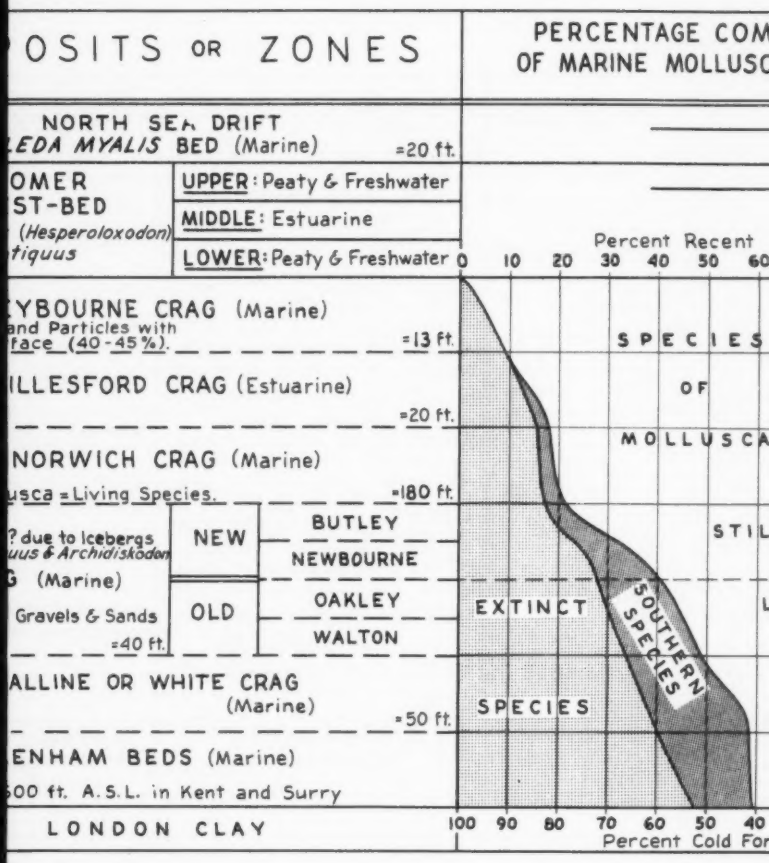
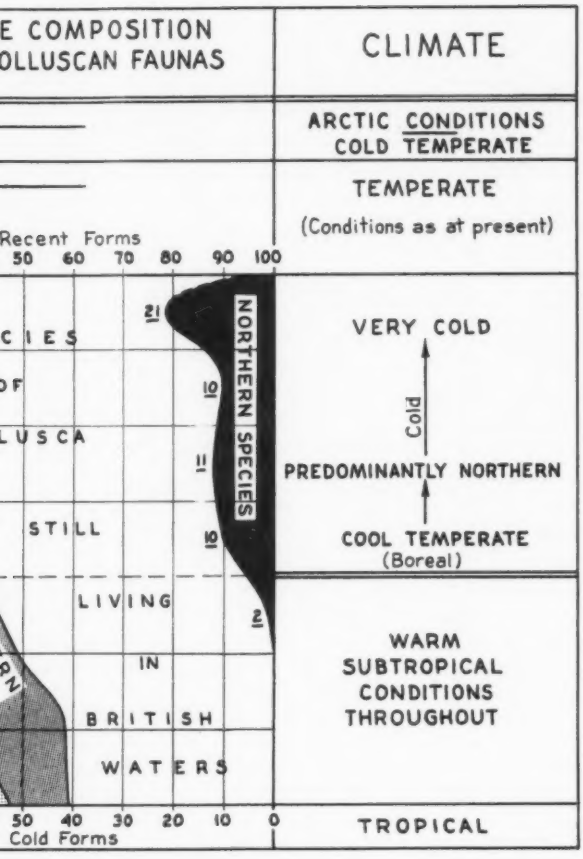


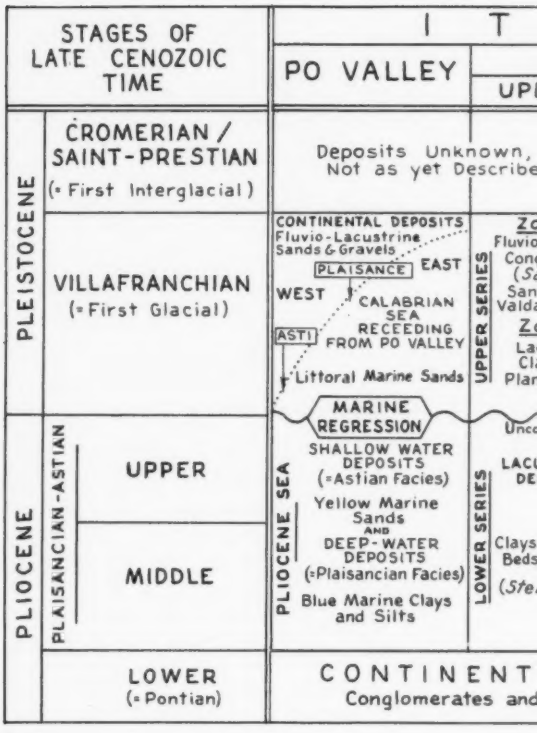
FIG. 7.—Chart showing the sequence of the East Anglian
1922; Boswell, 1936; Hopwood, 1936, pp. 900–902; 1940, p. 8



e East Anglian crag deposits and the percentage composition of their contained invertebrate m
02; 1940, p. 81; Zeuner, 1937; 1945, p. 106; Cailleux, 1942, pp. 113 and 126; and Flint, 1947, t



ertebrate marine faunas. (Based on Davies, 1934, fig. 28; also Osborn, nt, 1947, table 11.)


STAGES OF LATE CENOZOIC TIME
I T
PO VALLEY
UPI
PLEISTOCENE
CROMERIAN / SAINT-PRESTIAN
(= First Interglacial)
Deposits Unknown, Not as yet Describe
VILLAFRANCHIAN
(= First Glacial)
CONTINENTAL DEPOSITS
Fluvio-Lacustrine Sands & Gravels
PLAISANCE
EAST
WEST
CALABRIAN SEA RECEDING FROM PO VALLEY
ASTI
Littoral Marine Sands
UPPER SERIES
Zo
Fluvio
Cong
(Sa
San
Valda
Zo
La
Cl
Plan
MARINE REGRESSION
Unco
PLIOCENE
PLAISANCIAN-ASTIAN
UPPER
SHALLOW WATER DEPOSITS
(=Astian Facies)
Yellow Marine Sands
AND
DEEP-WATER DEPOSITS
(=Plaisancian Facies)
MIDDLE
Blue Marine Clays and Silts
PLIOCENE SEA
LOWER SERIES
LACU
DE
Clays
Beds
(Ste
LOWER
(= Pontian)
CONTINENT
Conglomerates and

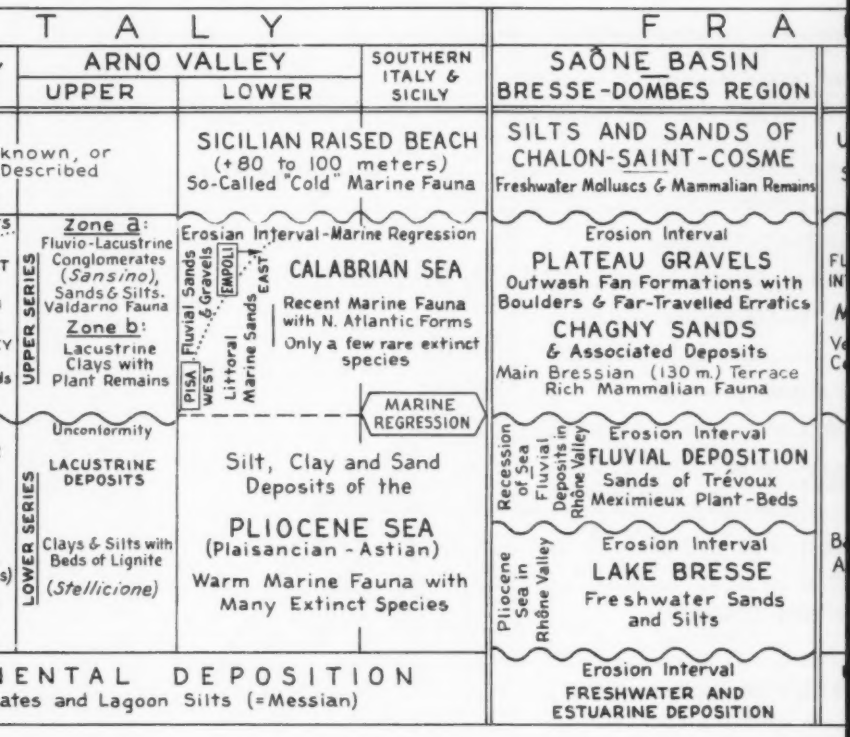


FIG. 8.—Correlation of late Cenozoic horizons in southern and south

F R A N C E		VERTEBRATE PALAEONTOLOGY	CLIMA
SAÔNE BASIN SSE-DOMBES REGION	PUY-EN-VELAY BASIN (HAUTE-LOIRE)		
TS AND SANDS OF ALON-SAINT-COSME ter Molluscs & Mammalian Remains	UPPER SANDS AND GRAVELS SAINZELLES & LA MALOUTEYRE	CROMER FOREST-BED/ SAINT-PREST FAUNA with *Elephas* (*Hesperoloxodon*) *antiquus*	Conditions Similar to th Prevailing at P
Erosion Interval LATEAU GRAVELS ash Fan Formations with ers & Far-Travelled Erratics CHAGNY SANDS & Associated Deposits Bressian (130 m.) Terrace Rich Mammalian Fauna	Erosion Interval FLUVIAL SANDS, GRAVELS & CLAYS INTERCALATED WITH BASALTIC FLOWS MASTODON SANDS OF PUY Velay Fauna: Vialette, Crozas, etc. Ceyssac: Diatomaceous Clays with Plant Remains	VILLAFRANCHIAN FAUNA ELEPHAS — *Archidiskodon* Group EQUUS — Zebrine Group LEPTOBOS — Primitive Cattle	CLIMATE CO AND WETT THAN AT PRE First Stage Continental Glac
Deposits in Rhône Valley Erosion Interval FLUVIAL DEPOSITION Sands of Trévoux Meximieux Plant-Beds	Prolonged Erosion Interval VOLCANIC ERUPTIONS CONTINUED Basalts, Andesites, Phonolites etc. Ash Layers with Plant Remains	MONTPELLIER AND ROUSSILLON (PERPIGNAN) FAUNAS Mammalian Faunas of this Stage are very poorly Represented in Europe	WARM CLIM Tropical t Subtropic Condition
Erosion Interval LAKE BRESSE Freshwater Sands and Silts			
Erosion Interval FRESHWATER AND ESTUARINE DEPOSITION	FIRST PHASE OF VOLCANIC ACTIVITY ASSOCIATED WITH TECTONIC MOVEMENTS	HIPPARION-FAUNA	SAVANNAH CLI Tropical to Subt Condition

ozoic horizons in southern and southwestern Europe

LIMATE

nditions very
ilar to those
iling at Present

ATE COLDER
ND WETTER
AT PRESENT

Stage of
ental Glaciation

RM CLIMATE

opical to
btropical
onditions

NAH CLIMATE
l to Subtropical
Conditions

is also indicated by the complete absence of northern species of Foraminifera collected by Ruggieri (1944, pp. 109–110) in the Imola region, southeast of Bologna. The discovery of a rich pollniferous deposit in a definitely Sicilian context would provide the necessary data to solve this problem. In the meantime, the evidence to date certainly does not substantiate the argument that climatic conditions in Italy during Sicilian times were markedly cooler than those of the present day.

At three localities in Sicily—Conca d'Oro di Palermo, in the vicinity of Taormina at the base of Mount Etna, and Mortelle (fig. 6), a small village near Messina (Gignoux, 1911, p. 128; 1913, pp. 615–618; 1922, p. 1478; Depéret, Mayet, and Roman, 1923, pp. 171–172) —this horizon, which is represented in the Messina region by a littoral conglomerate cut into the inclined Calabrian beds, has yielded remains of *Elephas* (*Hesperoloxodon*) *antiquus* Falc. Other finds of this ancient, African type of elephant have likewise been recorded from Sicilian deposits on the Italian mainland —near Reggio di Calabria (three localities), in the Leghorn region, and at San Severo, near Foggia.[21] Since the San Severo find, a very typical M_2 of *E. antiquus*, was discovered below a thickness of some 14 meters of marine gravels of the Sicilian stage, it is difficult to avoid the conclusion that the latter is of Lower Pleistocene (first interglacial) age. In any case, there seems to be absolutely no basis for the contention that the Sicilian stage should be placed in the Pliocene epoch.

[21] Gignoux, 1913, pp. 280 and 615; Depéret, Mayet, and Roman, 1923, p. 172. The abundant remains of *E. antiquus* Falc. from the Sicilian deposits of the Leghorn region are stated to occur in association with *Hippopotamus*.

THE SAÔNE BASIN OF EASTERN FRANCE

In the Saône Basin of eastern France, which is comprised of the Bresse region and the Pays-des-Dombes, the late Cenozoic stratigraphic sequence was worked out in great detail by Delafond and Depéret. Although several important contributions have subsequently appeared, the main succession of five Pliocene and Lower Pleistocene geological and paleontological events, as established by these eminent authorities, still remains substantially as they set it forth in their classic monograph, published in 1893 (cf. Béroud, 1909; Roman, 1929, p. 138; Pelletier, 1946*a*, pp. 131–138). As in the upper Arno Valley, the late Cenozoic deposits of this area consist of two main series of deposits, and these occupy nearly the entire extent of the basin. The older series is almost exclusively lacustrine, whereas the younger beds were for the most part laid down under fluvial conditions (fig. 2).

Between the Lower Pliocene freshwater sands, which yield a very typical Pontian (*Hipparion*) fauna (Depéret, 1887, pp. 104–112; 1894*c*, pp. 715–718; Delafond and Depéret, 1893, pp. 41–56; Viret, 1947), and the overlying Middle Pliocene lacustrine sediments, there is a marked disconformity caused by uplift and erosion. But the Middle Pliocene was everywhere a stage of depression and marine dominance, when the sea extended up the Rhône Valley to the vicinity of Givors within 20 km. of Lyons (see Fontannes, 1879–1882, especially the large folding map in vol. 2) and the entire Bresse-Dombes region was occupied by a lake,[22] as shown in figure 1. The beds laid

[22] This Middle Pliocene Lake of Bresse, which was over 200 km. long, emptied into the narrow marine "ria" of the Rhône Valley south of Lyons (see Tardy, 1883, pp. 562–564; 1886, pp. 91–101; Depéret, 1895, p. 445; Denizot, 1934, p. 642;

down in the latter area at this time contain a rich molluscan fauna of Levantine facies, as well as a mammalian assemblage[23] composed in the main of new invading types of animals not found in the earlier deposit.

At the end of the Middle Pliocene a new phase of orogenic movements connected with mountain-building processes led to the partial withdrawal of the sea from the Rhodanian Gulf and an erosion interval in the Bresse-Dombes Basin. In the valleys cut by the rivers at the expense of the Middle Pliocene lacustine beds during this interval, a dominantly fluviatile facies—sands, gravels, and tuffs—was accumulated (Delafond and Depéret, 1893, pp. 180–185). At Trévoux, an important locality of this stage on the Saône River, typically Upper Pliocene mammalian remains, mollusks, and imprints of leaves (Delafond, 1885; Depéret, 1889, 1894*b*, pp. 625–626) have been recorded, and similar deposits occur elsewhere in the region. The fossil vertebrates from these beds[24] have direct affinities with the well-documented contemporary assemblages of Montpellier and Roussillon (Perpignan) in southern France, the classic localities for Dépéret's "Astian fauna." In the lower valley of the Ain, in the extreme southeastern portion of the Dombes region, a tremendously rich flora of this stage (Delafond, 1886*a;* Delafond and Depéret, 1893, pp. 186–187; Depéret, 1894*a;* Roman, 1936*a*, p. 162; 1936*b*, p. 186) has been described in detail from the locality of Meximieux.[25] These Meximieux plant beds (fig. 9), which have become classic, were laid down under conditions of a warm, subtropical climate, when the prevailing conditions in the Bresse-Dombes Basin were very similar to those obtaining at present in the lower Mississippi Valley. With regard to southeastern France, Depape (1928, p. 81) has calculated that, during Upper Pliocene times, the 20° C (68° F.) isotherm passed through the general region of Avignon, while in the slightly more northerly Bresse-Dombes Basin the annual mean was between 17° and 18° C. (62°–64° F.). Thus the thick Upper Pliocene sand deposits, pebbly beds, and tuffaceous layers of the Saône region were accumulated during a period when the climate in the basin averaged some 5°.5 or 6° C. warmer than it does at present.

The next series of beds, which belong to the Villafranchian stage, appear to represent an abrupt break in the sedimentation of the Bresse-Dombes region. The characteristic deposits consist of very much coarser sediments; these occur in the form of huge terrace and fan formations, which are found in all the main valleys. They issue from the flanking massifs—those of Beaujolais and Bourgogne on the west and the Jura on the east—and disconformably overlie the older terrain.[26] These fluvial sands and

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Roman, 1936*a*, p. 160). As Gignoux (1943, p. 555) has observed, the situation during Middle Pliocene times in the Saône Basin is very reminiscent of the conditions which prevailed in the intra-Apennine depressions of Italy; the latter were also occupied by lakes during an otherwise dominantly marine stage.

[23] For lists see Depéret, 1893, p. 534; Delafond and Depéret, 1893, pp. 124–125; Viret, 1939; Pelletier, 1946*b*.

[24] For lists see Depéret, 1893, p. 535; Delafond and Depéret, 1893, pp. 191–197.

[25] De Saporta and Marion, 1872; De Saporta, 1873, pp. 217–219; 1879, pp. 322–337; Depape, 1928, p. 47. According to Laurent (1911, p. 90), an identical flora, also of Upper Pliocene age, occurs in a series of tuff deposits exposed not far from Belley (Ain), in the valley of the Furans River, a tributary of the Rhône.

[26] Delafond, 1886*b*, p. 69. For these widespread and very characteristic deposits of the Saône Basin the term "Bressian Conglomerate" (*Conglomérat*

gravels, which Depéret and Penck considered to be the equivalent of the *älterer Deckenschotter* of the northern Alps (Depéret, 1898, p. 423; Penck and Brückner, 1909, p. 648), are very extensive; they not only cover almost the entire Bresse-Dombes Basin (fig. 10) but are also found throughout the valley of the Rhône River and its tributaries south of Lyons.[27]

Ambérieu, a town located 12½ km. northeast of Meximieux, several fine exposures of the gravels of this stage have been studied in detail by Boistel (1894, p. 300; 1898*a*; pp. 31–33; 1902, pp. 138–139), who states that, although they have been much reduced by erosion, they once attained a thickness of over 40 meters at several places. Indeed, Depéret (1885*c*,

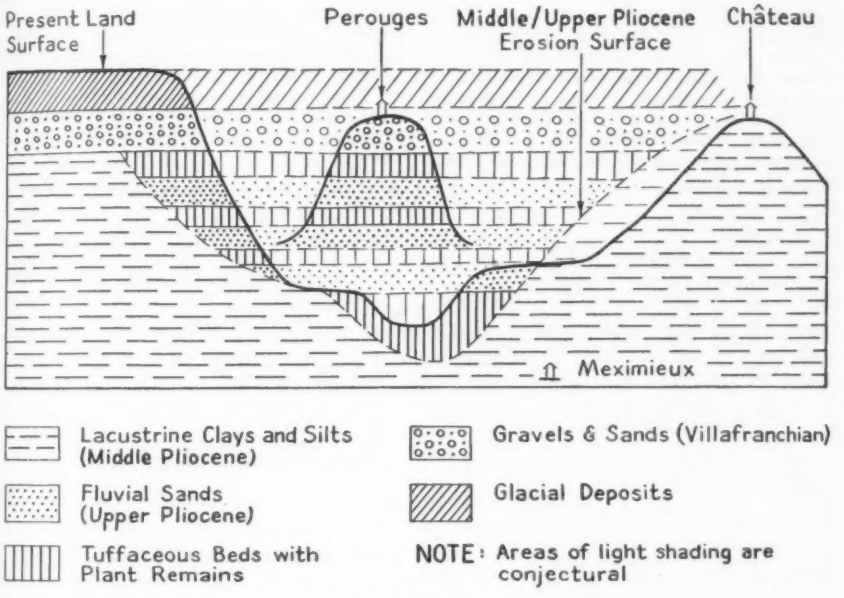

FIG. 9.—Schematic section of the deposits near Meximieux (Ain), showing the stratigraphic position of the Upper Pliocene tuffaceous beds containing abundant plant remains. (After Delafond and Depéret, 1893, fig. 43.)

Along the foothills of the southern Jura, from Bourg-en-Bresse south to the Rhône Valley, these fluvial deposits, which contain numerous pebbles of Alpine origin up to 20 cm. in diameter, form a more or less continuous sheet (Depéret, 1885*c*, p. 122; 1898, p. 422; Delafond, 1889, pp. 7–8; Boistel, 1898*b*, pp. 57–59). Near

p. 123) records a section near Montluel (see fig. 1), where he studied a vertical

Bressan) was originally proposed (cf. Benoît, 1858, p. 328; Fontannes, 1885, p. 60; Depéret, 1888), but it was subsequently abandoned.

[27] The Plateau Gravels, or *Cailloutis des Plateaux*, of the Lyons region and Bas Dauphiné (forests of Chambaran and Bonnevaux), as well as elsewhere in the lower Rhône Valley, which are also of Villafranchian age, likewise constitute an immense surface of alluvial deposits that has been described by many authors (cf. Fontannes, 1884; Riche, 1886, 1887, 1888; Depéret, 1895, p. 445; 1913, pp. 533–534; Kilian, 1902, pp. 160–161; 1911, p. 98; Martin, 1905–1906, pp. 146–157; De Lamothe, 1906, p. 1104; 1910 pp. 806–807; 1915, pp. 70–87; 1921, pp. 105–

section of these gravels approximately 80 meters high. According to this latter authority, it is evident on the basis of the thickness and extent of this vast sheet that it represents the eroded remnant of an alluvial terrace of the ancient Rhône which is also Viret's (1926, p. 55; 1931,

114; Kilian and Gignoux, 1910, pp. 1023–1024; 1911, pp. 203–210; Briquet, 1911, pp. 37–39; Faucher, 1913; Kilian and Révil, 1917–1918, pp. 213–235; Denizot, 1933, pp. 560–573; 1934, pp. 615–620; 1939, p. 125). These thick *Elephas* (*Archidiskodon*) *meridionalis*-bearing gravels, regarded by Penck (with Brückner, 1909, pp. 649–659; see also Schaudel, 1907, pp. 139–155) as corresponding in the main with the *älterer Deckenschotter* of the region to the north of the Alps, presumably are remnants of the outwash fans of a glaciation, the moraines of which have been long since dismantled as the result of subsequent erosion and extensive glaciation. Indeed, as Bourdier has recently stated (1946, p. 339; see also Depéret, 1922, p. 1419), the existence of actual morainic deposits attributable to the Günz stage in the French Alps is a very problematic matter. At the locality of La Croix-Thoré in the commune of Saint-Vallier (Drôme), however, the Plateau Gravels appear to be associated with an ancient calcareous loess (Bourdier, 1942, p. 423; 1947) from which Viret has recovered a very rich assemblage of mammalian remains, including *Equus stenonis*, *Crocuta* cfr. *perrieri*, and numerous species of typically Villafranchian artiodactyls. Not only does this evidence suggest that a Villafranchian fauna was living in southeastern France under the periglacial conditions of the Günz glacial stage, but also that the Plateau Gravels (*Cailloutis des Plateaux*) should be included in the Pleistocene.

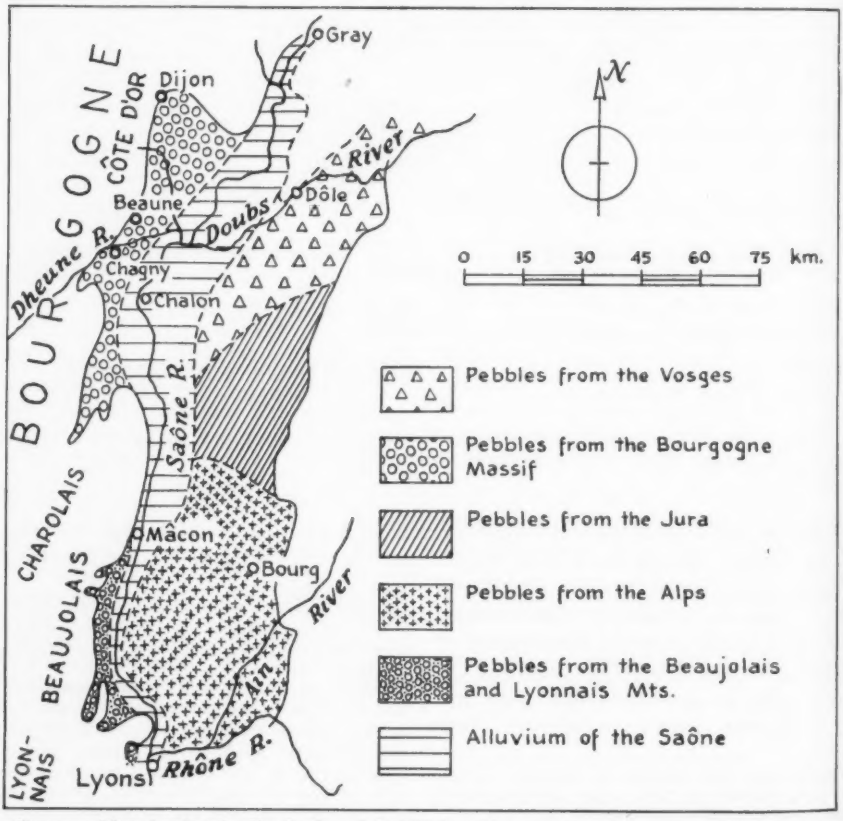

FIG. 10.—Map showing the distribution of the Villafranchian gravels and the source of their main constituents in the Saône (Bresse-Dombes) Basin. (After Delafond and Depéret, 1893, fig. 43.)

p. 168) opinion. This was therefore an intensive and widespread stage of alluviation, when all the streams issuing from the highlands were rapidly eroding the beds of their upper courses and transporting this material to the adjacent lowlands and plains, where it was deposited. As Delafond and Depéret (1893, p. 202) point out, this was an unprecedented period of erosion and deposition, which far exceeds in importance similar events associated with subsequent, and demonstrably Pleistocene, stages in the highlands of the western Alps and those of the Jura.[28]

It seems probable that this abrupt change in the depositional processes operating in this region is connected with a major climatic break that resulted in the first advance of the Alpine ice-sheets. Journaux (1948, p. 70) has recently reported definite signs of cryoturbation (*or* congeliturbation, see Bryan, 1946, p. 633; 1948) caused by frost action contemporaneous with the deposition of the huge outwash fans of this stage found along the base of the eastern flanks of the Côte d'Or, south of Dijon. Furthermore, in these fresh-water Villafranchian sediments, the constituents of which consist predominantly of erratics from the Vosges, Jura, and Alpine areas (fig. 10), angular and relatively unrolled boulders of Alpine origin, up to 50 cm. in diameter, have been reported (Boistel, 1902, p. 157; Journaux, 1948, p. 69). This evidence demonstrates that the deposits in question were accumulated under torrential conditions, when the climate was colder than it is at present in this region and when the transporting power of the streams was greatly in excess of what it has been during Recent times.

Now the relative age of these high-level gravel and sand deposits is established beyond all cavil on the basis of the very clear paleontological evidence from a great number of localities, where remains of characteristic Villafranchian mammals have been collected in the Bresse-Dombes region.[29] Especially along the western border of the basin, in the foothills of the Beaujolais and Bourgogne Mountains, there exist numerous exposures revealing the fact that the high-level (including the so-called plateau and intermediate) gravel horizons occur at progressively lower levels, until in the region between Chalon-sur-Saône and Dijon—an area known as the Côte d'Or—they actually merge with the main Bressian (130-meter) terrace, which carries the famous *Sables de Chagny* (Delafond and Depéret, 1893, p. 213; Depéret, 1894*b*, pp. 624–625; Mayet, 1922; Depéret, Mayet, and Roman, 1923, p. 20).

This latter bed is of fluvial origin, and the orientation of its constituents is in accord with the axis of the present valleys, according to Journaux (1948, p. 69). Extensive exposures of the *Sables de Chagny* occur in the valley of the Dheune, but in the main this formation is the basal portion of a very high (130-meter) terrace, which had already been strongly and unevenly eroded by first interglacial times. As shown on the section (fig. 11), it is consequently overlain at many localities by the sands and gravels of the 30-meter terrace of the Saône, in which remains of *Elephas* (*Mammuthus*) *primigenius* and Reindeer have been recorded (Delafond and Depéret, 1893, pp. 279–284; Changarnier, 1908;

[28] Cf. Fontannes, 1885, p. 65. Very similar torrential fan deposits, overlying Pliocene marine strata, have recently been described in the region around Figueras, just south of the Pyrenees in the extreme northeastern portion of Spain (Ribera-Faig, 1945).

[29] Depéret, 1909, pp. 140–141, where a list of the sites in question will be found; see also Delafond, 1879, p. 933; Depéret, 1885*a*, p. 256; 1885*c*, p. 122.

Depéret, Mayet, and Roman, 1923, pp. 20–21; Viret, 1926, p. 55; 1931, pp. 168–169). Although the main deposits of the *Sables de Chagny* are found on the right (west) bank of the Saône in the Côte d'Or–Beaujolais-Bourgogne region, it is also of sporadic occurrence on the left (east) bank of the river, as has been observed by Viret (1930) and Rouyer (1930).

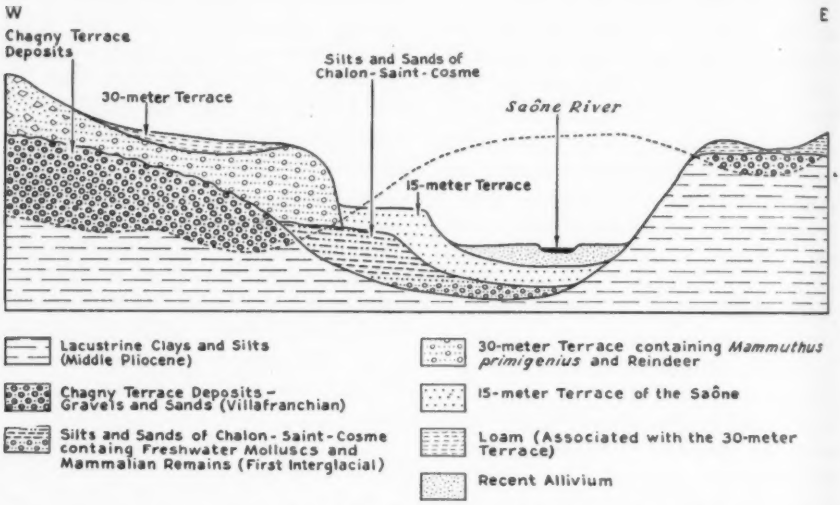
W
E
Chagny Terrace Deposits
Silts and Sands of Chalon-Saint-Cosme
30-meter Terrace
Saône River
15-meter Terrace
Lacustrine Clays and Silts (Middle Pliocene)
Chagny Terrace Deposits – Gravels and Sands (Villafranchian)
Silts and Sands of Chalon-Saint-Cosme containg Freshwater Molluscs and Mammalian Remains (First Interglacial)
30-meter Terrace containing Mammuthus primigenius and Reindeer
15-meter Terrace of the Saône
Loam (Associated with the 30-meter Terrace)
Recent Allivium

FIG. 11.—Diagrammatic section of the Saône Valley near Chalon, showing the stratigraphic position of the Chagny terrace deposits in relation to the silts and sands of Chalon-Saint-Cosme. (After Viret, 1931.)

An extremely rich mammalian assemblage with incontestably Villafranchian affinities is known from the sandy deposits of this main Bressian terrace in the vicinity of Chagny,[30] a town in the Saône Valley, as well as at a great number of localities north of Chagny, where rich collections of fossil bones were recovered when the railway from Beaune to Dijon was constructed during the 1880's.[31] These ferruginous sands and gravels have a very extensive distribution throughout the Bresse-Dombes Basin. Because they contain *Elephas* (*Archidiskodon*) *planifrons* Falc. (in association with the last of the mastodons), *Leptobos etruscus* Falc. and *Equus stenonis* Cocchi, as well as other forms known from the classic localities of the Asti, Val d'Arno, and Auvergne regions, there is absolutely no question concerning the paleontological affinities of this horizon. The Chagny locality provides further cognate proof that, as in the case of the Po and upper Val d'Arno Valley areas, the deposits of the Villafranchian stage are superposed

[30] Depéret, 1885*a*, p. 261–262; 1893, p. 535; Tournouër, 1866, pp. 787–788; Delafond and Depéret, 1893, pp. 231–329; Haug, 1911, p. 1844; Mayet, Nugue, and Dareste de la Chavanne, 1920*a*, *b*; Mayet, 1922; Depéret, Mayet, and Roman, 1923, pp. 22–24; Nugue, 1924; De Prunelé, 1924; Osborn, 1942, pp. 961–963. For detailed descriptions of the main fossil locality (Bellecroix) at Chagny see Depéret and Mazeran, 1920; Depéret, Mayet, and Roman, 1923, pp. 17–22.

[31] Tardy, 1884, p. 718; Parandier, 1891; Delafond and Depéret, 1893, p. 215; Changarnier, 1908, pp. 147–150 (list of localities); Depéret and Mazeran, 1920, pp. 307–308; Depéret, Mayet, and Roman, 1923, p. 21.

directly on those of the Upper Pliocene, although the two deposits are separated from each other by a period of unknown duration, when uplift and erosion occurred. Its marine equivalent, Calabrian, is not represented in southern France,[32] where the deposits of this stage in the lower Rhône Valley and in Languedoc are all of continental facies.

The formation of the Chagny, or main Bressian, terrace and associated deposits in the Saône Basin was followed by an important phase of erosion and valley-cutting, during which the Rhône, Ain, Saône, and other rivers in the region lowered their beds 40–50 meters (Delafond and Depéret, 1893, p. 241). There ensued an aggradational stage when fine-grained sediments—silts, sands, and clays—were laid down. These are known as the Silts and Sands of Chalon-Saint-Cosme,[33] and they are found at an average height of approximately 12 meters above the level of the present streams, although they have been recorded up to a maximum height of 25 meters (Delafond and Depéret, 1893, p. 242; Depéret and Mazeran, 1920, p. 306; Viret, 1931, p. 171). That a regime in part at least of lacustrine deposition was under way in the Chalon region is indicated by the fine sections investigated by Viret (1926, pp. 55–56; 1931, p. 171), in which horizons of finely bedded, laminated clays, typical of lake deposits and recalling varved sediments, were exposed. In the vicinity of Chalon-sur-Saône (fig. 11), this so-called "Saint-Cosme terrace" has yielded remains of a typical first interglacial (Saint-Prestian–Cromer Forest-Bed) mammalian fauna,[34] including true *Bos*, *Megaceros hibernicus* Owen, and *Trogonterium cuvieri* Fisch. The mollusks, described by Delafond and Depéret (1893, pp. 253–256), from the clay pits at Saint-Cosme clearly demonstrate that the temperate conditions of a climate very similar to that of the region at present prevailed at the time that the sediments in question were being accumulated. The fact that in the Rhône Valley the outwash gravels and morainic debris of a major ice advance (cf. Delafond and Depéret, 1893, p. 247) directly overlie these Saint-Cosme sands and silts is further convincing evidence that the latter stratum was formed during interglacial times. In any case, the Chalon–Saint-Cosme locality is of great importance from a stratigraphic point of view; it is one of the very few sites in France where a fauna of the first interglacial stage directly overlies demonstrably Villafranchian deposits. Furthermore, just as in Italy and Sicily, an erosion interval separates the two beds.

[32] With the possible exception of the lower valley of the Var (cf. Depéret and Caziot, 1903, pp. 322–326; Bertrand, 1904; Gignoux, 1911, p. 129; 1913, p. 336). However, marine sands and clays of the Plaisancian-Astian are extensively developed all along the coast of Provence (Lutaud, 1924, pp. 33–35). At the classic locality for vertebrate mammalian remains of the Upper Pliocene stage (Depéret's "Astian fauna"), situated near Montpellier (Hérault), the deposits have been intensively studied by the geologist Viguier (1889, pp. 390–399; 1890). Here the uppermost horizon consists of fluvial gravels, which ravine the fresh-water sands and silts of the Upper Pliocene and which have yielded typical remains of the Villafranchian elephant, *E.* (*Archidiskodon*) *meridionalis* Nesti. Furthermore, everywhere on the Roussillon plain heavily weathered sands and gravels of continental facies disconformably overlie the Upper Pliocene lacustrine series (Bourcart 1939, p. 166; 1945; pp. 44–45). It is therefore apparent that in southern France the continental Villafranchian deposits are directly superposed on those of the Upper Pliocene, although separated from them by a disconformity, just as they are in the Saône Basin, the main valley of the Rhone, the Po region, and the Arno Valley.

[33] Delafond (1886*b*, p. 80; 1890) was the first geologist to recognize this horizon as an independent stratigraphic unit in the late Cenozoic sequence of the Bresse-Dombes Basin.

[34] For lists see Adenot, 1899; Delafond and Depéret, 1893, pp. 248–253; Nugue, 1907; Haug, 1911, p. 1844; Depéret, Mayet, and Roman, 1923, p. 64.

BASIN OF PUY-EN-VELAY (HAUTE-LOIRE)

For well over a century the importance of the basin of Puy-en-Velay has been known to collectors of fossil vertebrates (cf. Robert, 1829; Aymard, 1853) as well as to students of vulcanism; but no comprehensive work dealing with the geology of the region as a whole appeared until 1892, when Marcellin Boule's famous *Description géologique du Velay* was published. This has remained the standard work on the area ever since.

The Pliocene deposits of the Puy-en-Velay region are dominantly of volcanic origin, which at one locality in the Mézenc Massif, southeast of Le Puy, have yielded a typically Pontian (Lower Pliocene) fauna (Boule, 1892, pp. 112–113). Although no vertebrate fossils have thus far been discovered in the Lower or Middle Pliocene stages in the Puy Basin proper, plant remains have been found at several localities in beds of this age. These have been intensively studied by De Saporta and Boulay (De Saporta, 1879, p. 337; Boulay, 1887; De Saporta, in Boule, 1892, pp. 116–118). The flora demonstrates that warm subtropical conditions prevailed in central France at this time. The relative poverty of Middle Pliocene[35] deposits in the Puy-en-Velay area as a whole can be explained, according to Boule (1892, p. 167), as the result of the erosion and down-cutting of the basin by the Loire River and its tributaries during the ensuing stage—the Upper Pliocene—on the basis of the stratigraphic scheme adopted in this paper.

Following this prolonged phase of volcanic activity and erosion, an extremely important series of ferruginous-colored, alluvial deposits intercalated with volcanic material was accumulated, considerable portions of which still remain

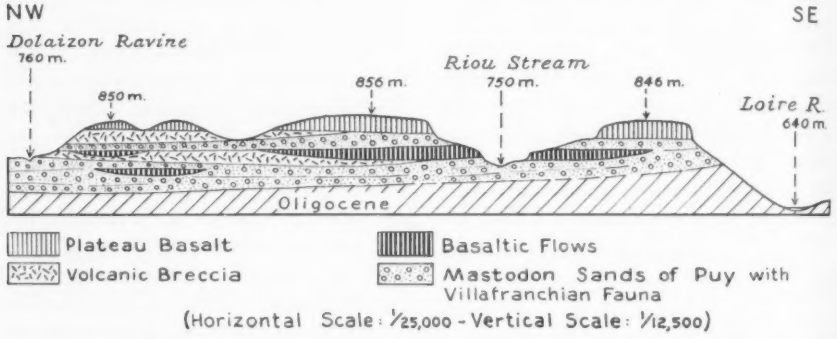

FIG. 12.—Section through the Cenozoic deposits of the Puy-en-Velay Basin between the Dolaizon ravine and the valley of the Loire. (After Boule, 1892, fig. 49.)

(fig. 12).[36] These fresh-water sediments, which are commonly cross-bedded and which are known collectively as the *Sables à Mastodontes du Puy*, consist in the main of fine micaceous and quartzitic sands; but gravel layers, lenses of rolled pebbles, and beds of varicolored clays also occur. At several points this bed, the surface of which is some 200 meters above the Loire, attains a total thickness of

[35] This corresponds to the *Pliocène inférieur* in Boule's stratigraphic scheme (cf. Boule, 1892; 1893*a*, *c*).

[36] For a detailed description of this interesting series see Boule, 1892, pp. 168–201; see also Chaput, 1917, pp. 59–60; 1919, pp. 85–86; Baulig, 1928, p. 219.

nearly 100 meters; from it a rich Villafranchian mammalian fauna has been described. Although at two localities the clay beds are prolific of diatoms and plant remains, no detailed study of them has ever been made. Boule gives the identifications of the diatoms which he collected from the clay deposits exposed near the town of Ceyssac (fig. 13), in the central portion of the basin, some 4 km. due west of Le Puy, but makes no further comments (Boule, 1892, p. 187). His list, however, was studied by Dr. Roy M. Whelden, of the staff of the Farlow Herbarium, Harvard University, who states that five of the species represented: *Melosira arenaria* Moore, *M. granulata* (Ehrenb.) Ralfs., *Navicula nobilis* (Ehrenb.) Kütz., *Stephanodiscus astraea* (Ehrenb.) Grun., and *Tetracyclus emarginatus* (Ehrenb.) W. Sm. are forms most frequently found in cold water, whereas, with respect to the assemblage as a whole, he feels that "one might safely conclude that the environment in which these diatoms grew was at least cool temperate, and more probably was decidedly cold." Dr. Whelden's observations are in complete agreement with E. Haug's statement (1911, p. 1821) that the collection of these microscopic organisms from Ceyssac reveals a very marked predominance of cold species.[37]

In addition to diatoms, the Ceyssac clay beds have yielded a rich collection of fossil plant remains.[38] As De Sapotra and Depape have pointed out, in comparison with the older floras of the Massif Central, on the one hand, and those of the Rhône and Ain valleys, on the other, the Ceyssac flora offers a very striking contrast; for, in it, all the tropical to subtropical forms (*Bambusa*, *Magnolia*, *Sabal*, *Zelkovia*, *Sassafras*, etc.) have completely disappeared; indeed, only a very small number of forms reported from any of the demonstrably earlier localities has survived. The Ceyssac flora, which included *Pinus*, *Picea*, *Abies*, *Vaccinium*, *Carpinus*, *Acer*, *Alnus*, *Ulmus*, *Populus*, *Salix*, *Fraxinus*, *Crataegus*, and *Pyrus*, indicates that during Villafranchian times the climate was somewhat wetter than it is in this region at present and, furthermore, that the mean annual temperature of the Puy-en-Velay Basin was approximately 10° C. (50° F.),

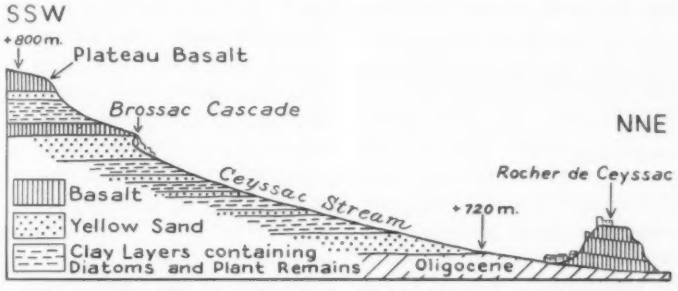

FIG. 13.—Geologic section showing the clay beds of Villafranchian age exposed near Ceyssac in the vicinity of Le Puy (Haute-Loire). (After Boule, 1892, fig. 49.)

[37] See also Lauby (1910, pp. 293–294), who mentions the interesting fact that certain "cold" types of fresh-water mollusks are fairly abundant at the Ceyssac locality. In this connection a study of the insect remains that occur in the Ceyssac clay beds (Boule, 1892, p. 185) would doubtlessly prove interesting.

[38] De Saporta, 1873, pp. 226–230; 1879, pp. 344–345; also Boule, 1892, pp. 185–187; Depéret, 1893, p. 527; Lauby, 1910, p. 293; Laurent and Marty, 1927, pp. 35–36; Depape, 1928, pp. 45 and 66–67.

or 2° C. cooler than the present mean (12° C. = 53°.6 F.) for this section of France (see Depape, 1928, p. 81). Therefore, an analysis of the flora is consistent in every respect with that derived from a study of the diatoms, although in neither case have the results been worked out on a numerical basis, according to the percentage frequencies of the various forms represented in terms of the total assemblage. The climatic interpretation based on the above is significant, however, because it implies that at the time of the accumulation of the Mastodon Sands of Puy-en-Velay the mean annual temperature in this basin was not only considerably colder (possibly as much as 8° C., or 14°.4 F. *lower*) than it has been previously during the Pliocene, but also it was some 2° C. (3°.6 F.) colder than it is on the average in this region at present. This evidence is in complete accord with the view expressed elsewhere, namely, that the Villafranchian in southern and western Europe represents a general phase when continental conditions prevailed,[39] when the sea was at a relatively low level, and when a glacial episode was already making itself manifest.

The fossil vertebrates known from the Mastodon Sands of Puy[40] were long regarded as demonstrating a Middle Pliocene age for this formation, but, because the animals represented constitute an assemblage that is incontestably Villafranchian (see Depéret, 1885*a*, p. 255; 1893, p. 536; Haug, 1911, p. 1821; Depéret, Mayet, and Roman, 1923, p. 34), this view is no longer tenable. Indeed, the fauna from the deposits in question is very similar in all its essential elements to those recorded from the classic sites of Perrier, Senèze, Chagny, the upper Arno and the Asti region (Depéret, 1893, p. 524; 1909). In spite of the fact that this fauna is known from a number of localities in the Puy Basin, it is surprising that no data have ever been published concerning the relative stratigraphy of the remains in question. As the *Sables à Mastodontes du Puy* are up to 100 meters thick, it is very probable that they cover the entire range of the Villafranchian, although they may have been laid down relatively rapidly in a crater lake. Until the Velay Basin is resurveyed and studied by a geologist in company with a paleontologist, however, there seems to be absolutely no justification for the contention that the Villafranchian stage can be subdivided into "early" and "late" substages on the basis of the evidence from this region.

In the Puy Basin, as in the case of the Saône Valley and the Italian peninsula, the Villafranchian stage was closed by uplift and erosion. According to Boule, it is probable that epeirogenic movements related to a new cycle of vulcanism occurred at this time, a view which is supported by the marked disconformity between the Mastodon Sands and the overlying volcanic flows, which is observable at many localities (Boule, 1889, p. 278; 1890, p. 948; 1892, pp. 200–202; 1893*d*, p. 518). The age of these eruptions is established on the basis of the paleontological evidence from La Malouteyre, on the northeastern flank of Mount Denise,[41] and Sainzelles, a small

[39] As pointed out on p. 399, the sea had completely withdrawn from the Rhône Valley by this time; Italy was likewise slowly emerging in a general north-to-south direction (see p. 390).

[40] For lists and descriptions see: Depéret, 1885*a*, p. 255; 1885*b*, p. 121; 1893, p. 536; 1907, p. 425; Boule, 1889, p. 275; 1892, p. 188; 1893*b*, p. 510; 1907; Laurent, 1907, p. 388; Haug, 1911, p. 1821; Depéret, Mayet, and Roman, 1923, pp. 35–37; Viret, 1942.

[41] The extinct volcanic cone of Mount Denise is located approximately 3 km. northwest of Le Puy.

village situated some 2½ km. northwest of Mount Denise. At La Malouteyre[42] the post-Villafranchian deposits are mainly of volcanic origin, and they have yielded a fauna which is definitely of first interglacial (Saint-Prestian) age.[43]

The fossil locality of Sainzelles (fig. 14), which was discovered nearly a hundred years ago (Aymard, 1853), consists of re-worked volcanic tuffs, apparently accumulated during and immediately subsequent to the erosion interval previously mentioned. These deposits are overlain by an extensive sheet of basalt; they may be easily distinguished from the Mastodon Sands by their grayish tint, by the large size of their constituent pebbles, and by the angular nature of the gravels which they contain. As Depéret originally pointed out,[44] the Sainzelles fauna is very close to that of Saint-Prest, near Chartres (Eure-et-Loire), on the one hand, and to the assemblage from Chalon–Saint-Cosme in the Saône Valley (see p. 399), on the other. Furthermore, at the latter locality, as well as in the Puy Basin, the beds yielding this first interglacial fauna occupy an analogous stratigraphic position with respect to the underlying deposits of the Villafranchian stage. It is to be regretted, however, that no plant remains have thus far been discovered at the Sainzelles–La Malouteyre localities of the Puy region, because it would be interesting to compare them with the earlier Villafranchian assemblage from Ceyssac. Nevertheless, as Haug (1911, p. 1822) has pointed out, the presence of Hippopotamus (*H. major* Cuv.) is very convincing evidence that, at the time that this bed was accumulating, a relatively warm interglacial climate prevailed in the region.

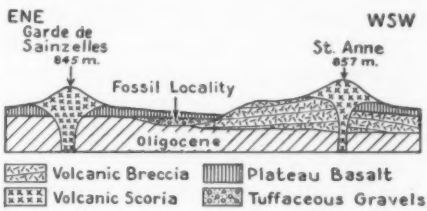

FIG. 14.—Section showing the stratigraphic position of the fossiliferous gravels at Sainzelles in the Puy-en-Velay Basin (Haute-Loire). (After Boule, 1892, fig. 60.)

CONCLUSIONS

The stratigraphic facts briefly outlined above are summarized in the table of figure 8; their significance with regard to the problem of the boundary between the Pliocene and the Pleistocene is at once apparent. Collectively they demonstrate the following:

1. That the deposits containing the classic Villafranchian fauna, considered as representing a Villafranchian stage, not only consistently overlie the Upper Pliocene in Europe but are also, in turn, immediately overlain by deposits that clearly belong to the first interglacial (Cromer Forest-Bed–Saint-Prestian) stage.
2. That, with the exception of peninsu-

[42] For references to the La Malouteyre fauna see Boule, 1889, p. 278; 1892, p. 202; 1893*d*, p. 518; Déperet, 1893, p. 536; 1909, p. 142; Haug, 1911, p. 1822; Depéret, Mayet, and Roman, 1923, pp. 38, 151.

[43] The fossil locality of Saint-Prest, situated in the valley of the Eure, 8 km. northeast of Chartres (Eure-et-Loire), has been described in detail by Denizot (1927; see also Haug, 1911, p. 1807; Depéret, Mayet, and Roman, 1923, pp. 60–62; Joleaud and Alimen, 1945, p. 95). The Saint-Prestian stage, which is the equivalent in France of the Cromer Forest-Bed horizon of southeastern England, was considered to be *Pliocène supérieur* by Boule (cf., e.g., Boule, 1892, p. 201, 1946, p. 165).

[44] Depéret, 1885*a*, p. 263; also Joleaud and Alimen, 1945, pp. 15 and 144. For lists of the Sainzelles fauna see Depéret, 1885*a*, p. 263; 1893, p. 536; 1909, p. 142; Boule, 1892, pp. 203–206 and 216; 1893*d*, p. 523; 1893*e*, p. 88; Haug, 1911, p. 1822; Depéret, Mayet, and Roman, 1923, pp. 62–63.

lar Italy, where a marine regime still prevailed, the deposits in question, which are of continental facies (relatively coarse fluvial and fluvio-lacustrine sands and gravels) represent a very marked break in the sedimentary cycle that had obtained in southern and southwestern Europe throughout the Middle and Upper Pliocene.

3. That during this part of the late Cenozoic period a marked lowering of the temperature occurred, which is consistent with the view that the Villafranchian deposits were laid down under the conditions of a pronounced climatic deterioration, which one would expect to accompany the onset of the first glacial stage.
4. That immediately prior to the initiation of this phase of climatic change a series of orogenic movements, which resulted in uplift and erosion, profoundly modified the entire Apennine tract. Furthermore, contemporary coastal deposits in Italy and Sicily prove that the Calabrian Sea was of a transgressive nature and that between the silts, sands, and clays of this stage and those of the Plaisancian-Astian series an important marine regression occurred.

With regard to the stratigraphic evidence as a whole, the sequence in southern and southwestern Europe, therefore, appears to be complete from the base of the Pliocene through to the end of the first interglacial stage. Each of the five major subdivisions (Pontian, Middle and Upper Pliocene, first glacial, and first interglacial stages) is represented by a series of geological horizons. Because there seems to be no doubt whatsoever concerning either the first interglacial dating of the Saint-Cosme–Sainzelles-Sicilian horizons or the pre-Pleistocene (i.e., Upper Pliocene) age of the Plaisancian-Astian and equivalent deposits, the Villafranchian beds should have been laid down during the part of late Cenozoic time separating these two latter stages. In other words, the stratigraphic evidence clearly indicates that the Villafranchian sediments of southern and southwestern Europe were accumulated during the first major glacial episode of late Cenozoic time. On this basis the Villafranchian is regarded by definition as marking the base of the Pleistocene rather than as representing the final stage of the Pliocene.

In view of the fact that the primary concept on which the science of geology is based is the Law of Superposition, one actually encounters considerable difficulty in retaining the Villafranchian stage in the Pliocene epoch; for, by so doing, the first glacial stage is represented stratigraphically only by a lacuna, with *no* deposits as yet reported from anywhere throughout the vast region under consideration which are referable to it. That such an event, which reflects a major climatic change of the first order of magnitude, should not have been widely recorded seems very hard to understand. Indeed, it would be difficult to attribute the huge torrential outwash fan deposits in the western Alpine forelands as due to any cause other than the relatively sudden impact of a glacial climate on the depositional processes operating in the region. With respect to the western portion of the Po Plain, this concept was originally propounded by Sacco (1890, p. 331) almost sixty years ago. In any case, the fact that a marked lowering of the temperature occurred at this time is proved by the following critical evidence: (*a*) the invasion of the Mediterranean Basin by "cold" boreal types of mollusks and Foraminifera, which represent up to

10 per cent of the total marine invertebrate fauna; (*b*) the plant remains from (1) Lodi in the northwestern section of the Po Plain, (2) various localities in the upper Val d'Arno where Zone b of the upper series of the basin sediments has been investigated, and (3) the Villafranchian clay beds exposed near Ceyssac in the Puy-en-Velay Basin of Haute-Loire; (*c*) the diatoms from the latter locality; and (*d*) the congeliturbated layers that have been recently described in the outwash fans of Villafranchian age in the Dijon region. From a chronologic point of view this series of events seems to be closely related and to be the result of the same general climatic change which led to the temporary establishment of a regime during which conditions were both cooler and wetter throughout southern and southwestern Europe than they are at present.

Immediately prior to the initiation of the Villafranchian-Calabrian stage in Italy, an important phase of orogenic movements occurred in the Apennines, which caused the Plaisancian-Astian beds to be uplifted and tilted and the formation of an erosion surface on those portions that were exposed. At the same time and apparently directly related to the mountain-building processes taking place in the Apennines, there was an abrupt break in the cycle of marine sedimentation which had been under way in the Mediterranean since the close of the Pontian. Although the evidence for this break is not everywhere apparent in the Messina region and Calabria, many localities elsewhere in Sicily, southern Italy, and along the Po Plain clearly demonstrate the transgressive nature of the Calabrian. This followed a phase of marine regression that marks the close of the Plaisancian-Astian series. In comparison with the total span of Pliocene and Pleistocene time, however, the interval represented by these latter events was presumably of short duration. Finally, the fact that Europe was invaded at this time by new forms of animals is regarded as significant, in view of the fact that, normally, large migrations of terrestial mammals are directly related to broad geographical changes.

This problem is vast, and it opens up intriguing fields for further studies. Among the projects that should be given high priorities in this connection are the following:

1. An investigation of the dates of the "late Pliocene" epeirogenic movements in the Alpine region, references to which in the literature are at present somewhat confusing in most cases and not clearly defined stratigraphically.
2. Further soil studies as a means of establishing the climatic conditions of the time of formation of these early deposits, as well as the processes by which they were formed.
3. A reinvestigation of the Ceyssac plant-bearing and diatomaceous deposit in the Puy-en-Velay Basin by modern paleobotanical methods.
4. A percentage frequency determination of the Calabrian and Sicilian molluscan faunas from localities selected to represent northern as well as southern Italy and Sicily.

It is further suggested that the actual beds from which the mammalian fossils have been collected in the past be carefully rechecked in the light of modern field methods. With the sole exceptions of the Senèze, Roca Neyra (Perrier), and Bellecroix (Chagny) localities, the paleontological material in general is rather vaguely grouped and referred to, both with regard to locality and stratum. Ap-

parently, the main objective of the early paleontologists was to amass as great a bulk as possible of fossil bones of various types, which may account for the fact that remains of small carnivores, rodents, insectivores, etc., are either very rare or supposedly nonexistent. The presence of these small forms would furnish critical climatic evidence in many cases. On the other hand, there is an abundance of large animals—proboscidians, rhinoceroses, Bovidae, horses, etc.—which are of little help for environmental studies. In only a very few cases, however, is the precise stratigraphic horizon of a given find indicated. As the deposits in the upper Val d'Arno and in the Puy-en-Velay Basin are extremely thick, they presumably represent considerable time. It is reasonable, therefore, to expect that some phylogenetic change in the fauna from base to top probably took place. In view of the fact that during the time represented by the stratigraphic break at the base of the Pleistocene many of the higher vertebrates seem to have been passing through an extremely critical evolutionary stage, this problem would almost certainly repay careful investigation. In the meantime the present data are not regarded as sufficient to permit subdivision of the Villafranchian stage, the beginning of which is believed to coincide with that of the Pleistocene epoch.

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Wind-faceted pebbles

GEOLOGICAL NOTES

SOME REMARKABLE WIND-FACETED PEBBLES FROM NEAR THE MOUTH OF THE UMHLANGAKULU RIVER, SOUTHERN NATAL, SOUTH AFRICA[1]

LESTER KING
University of Natal, Durban, South Africa

In years past I have made several field studies upon the cutting of facets on pebbles by the action of wind-driven sand, and particularly upon the number and direction of facets in relation to the dominant wind directions.

The approach has been from several angles. First, to evaluate the effect of original pebble shape, studies were made of angular material shed from a limestone outcrop upon the beach near the Ure River, Marlborough, New Zealand (King, 1936*a*). Here only two opposite wind directions along the beach could be effective for sand-blasting. Material was studied at all stages from freshly shed fragments to superbly faceted dreikanter and pyramidalgeschiebe. The conclusion was reached that, *with angular material*, original shape exercised considerable control over the final form assumed under sand-blasting.

Later, deposits near the mouth of the Clarence River in the same province (King, 1936*b*) (where, again, only two opposed wind directions could prove effective) afforded opportunity for the study of the relations of facets to dominant wind directions. Here river-worn, rounded boulders lay, abundantly faceted, on the raised beach. The conclusion reached was that *upon originally rounded materials* wind direction controls the orientation of facets.

In these and other cases (King, 1949), a major difficulty has been to demonstrate that the pebbles lay in relatively undisturbed condition and that they had undergone no rotation during their formation in either a horizontal or a vertical plane. Pebbles and facets were always recorded as completely as possible (King, 1936*b*); but, even so, the degree to which disturbance might enter, especially owing to the trampling of man and other animals, remained largely uncertain.

Wind-faceted lydianite pebbles projecting from a fossiliferous marine conglomerate of Cretaceous age just south of the Umhlangakulu River, southern Natal, are therefore of special interest. Here the conglomerate crops out along the present-day beach in such a manner that many of the pebbles, held fast by their nether surfaces in the Cretaceous matrix, have had their upper parts faceted by wind-driven sand. They cannot have been rotated at any time; study of their facet

[1] Manuscript received October 16, 1948.

PLATE 1

A–E, Wind-faceted stones, all oriented as found and with wind direction shown by arrow, from the beach south of Umhlangakulu River mouth, Natal, South Africa. The pebbles projected from the surface of a Cretaceous conglomerate in which they were embedded, fragments of which still adhere to them. Faceting is by wind-driven sand of the present beach. All facets are nearly at right angles to the only effective wind direction.

F, Crudely faceted pebble *in situ* in fossiliferous Cretaceous conglomerate. The faceting by wind is of recent date. Beach south of Umhlangakulu River mouth, Natal, South Africa.

directions may be expected to bear more than usual significance.

That the faceting is not of Cretaceous age but is of modern origin is clear because (*a*) none of the other pebbles in the body of the conglomerate bear any facets but, instead, are rounded and water-worn, and (*b*) none of the pebbles exposed superficially on the adjacent, wave-swept, tidal rock platform bear any facets. The faceting, let us repeat, is a product of the present situation on the existing wind-swept beach.

Only winds blowing from the southwest to the northeast, parallel with the trend of the beach, are effective because the landward side is cut off by high dunes, fixed by vegetation, and the winds off the sea lack the long fetch of sand necessary to faceting. Winds blowing from northeast to southwest are ineffective because the lagoonal outlet of the river traps any pronounced drift of sand from that direction.

The controls are thus stringent: the pebbles are held fast in matrix and are incapable of rotation, and there is but a single direction of effective sand-blast.

Unfortunately, the number of faceted pebbles is small, barely a score, though many more doubtless lie buried beneath the beach sand farther down the coast. Nevertheless, the facets are distinct and distinctive—no more than one to each faceted pebble, and every facet faces directly to the southwest, i.e., upwind. This is so whatever the orientation of the longer axes of the pebbles, so that in some cases the facets are placed laterally and in other cases "on the nose" of the pebbles. Some are intermediate in position.

Thus it appears that, on originally rounded material, effective wind direction alone is responsible for the direction in which facets are cut, also that the normal direction is at right angles to the wind. This conclusion is in harmony with the census of wind-faceted pebbles from the Clarence River mouth (King, 1936*b*) and the experiments of Schoewe (1932).

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GEOCHEMICAL RESEARCHES IN JAPAN[1]

KAZUO YAMASAKI

Nagoya University, Chikusa, Nagoya, Japan

Geochemical research in Japan began in 1913 when Y. Shibata spectrochemically analyzed naegite, a variety of zircon containing rare earths, thorium, and uranium. Shibata continued the research on Japanese minerals containing rare elements with collaborators and published the results in 1923. This was the first report on geochemical research in Japan, although the word "geochemistry" was not used.[2] These investigations were followed by those of Kimura, and since that time about forty reports have been published. Besides Kimura, S. Iimori and J. Takubo also studied the minerals containing rare elements. Iimori in particular studied the minerals found in Korea. The occurrences of the following minerals and

[1] Manuscript received October 27, 1948.

[2] The Japanese word *Tikyu Kagaku*, corresponding to "geochemistry" and meaning literally the "chemistry of the earth," was invented and used by Y. Shibata in 1926 for the first time.

the results of their analyses were described: allanite, autunite, beryl, calciogadolinite, columbite, enalite, euxenite, fergusonite, gadolinite, ilmenorutile, ishikawaite, pitchblende, samarskite, tantalite, thorogummite, torbernite, uraninite, xenotime, yttrialite, zircon.

The minor constituents of Japanese shales were studied by Minami in Göttingen. Volcanic rocks of volcanoes Asama, Izu-Oshima, etc., were analyzed by I. Iwasaki, and the mean composition of Japanese igneous rocks as determined by him is given in table 1. M. Ikawa studied plutonic rocks from the Dando District of central Japan, which resemble in geological and petrological properties the trondhjemites of Norway. Trace elements in Japanese soils, such as arsenic, iodine, and boron, were determined by K. Hirai. Recently, a very interesting aqueous inclusion was found in nepheline basalt in Iwami-Nagahama. This is thought to be the residual water from magmatic differentiation. There is a close resemblance between the chemical composition of this water and that of sea water.

The hydrosphere is better known than the lithosphere. The geochemical studies of hot springs in Japan were made by Kimura, Kuroda, Nakai, Okuno, Uzumasa, and others. Kimura and Kuroda analyzed more than three hundred spring waters spectrochemically and found the following trace elements: Ag, As, Au, B, Ba, Be, Bi, Co, Cs, Cu, Cr, Ga, Ge, Li, Mn, Mo, Ni, P, Pb, Sb, Sn, Sr, Ti, V, W, Zn, and Zr. The fluorine content of mineral springs was determined by Okuno, and their radioactivity was studied by Nakai and Kuroda. The heavy-water content of many kinds of natural waters was determined by Oana.

Chemical studies of Japanese lakes were made by the late S. Yoshimura in 1926. K. Sugawara also made a thorough study of lake metabolism. In 1928 I. Okada made a geochemical study of Dabsnor, a soda lake in Manchuria, and discovered gaylussite ($Na_2CO_3 \cdot CaCO_3 \cdot 5H_2O$) for the first time in the Orient. Extensive studies of brine from Szechwan Province in China were made by the late S. Goda in Shanghai. The chemical composition of river waters was studied by E. Kurashige and Y. Miyake.

Studies of the sea include the work of Y. Miyake, who analyzed the oceanic salts of the western Pacific Ocean and determined the vertical distribution of minor constituents in the Kurosio region. The radium content of the Yellow Sea and the China Sea was determined by Goda in 1930, and his results ($0.04 \sim 0.15 \times 10^{-12}$ gm radium/l) are in good agreement with the data of R. D. Evans. The radium content of ocean-bottom sediments of the western Pacific Ocean collected by the naval sounding ships was determined by H. Hamaguti. M. Ishibashi determined the quantities of trace elements, such as Au, Cs, Cu, Li, Pb, and Ra, in sea water.

TABLE 1

MEAN COMPOSITION OF JAPANESE IGNEOUS ROCKS

Oxide	Per Cent	Oxide	Per Cent
SiO_2	59.84	CaO	6.54
Al_2O_3	16.84	MgO	2.92
TiO_2	0.74	Na_2O	3.00
Fe_2O_3	2.80	K_2O	1.49
FeO	4.55	H_2O	1.28
MnO	0.21	P_2O_5	0.22

The study of the natural gases in Japan was first made in 1923 by N. Yamada in a search for helium. The highest helium content found was 0.3 per cent, but the quantity discharged was very small. The helium content of monazite, beryl, and other minerals was determined by J. Sasaki and B. Yamaguchi. Gases from fumaroles and mineral springs were studied by Noguti. The gaseous impurities contained in the air, such as ammonia, sulphur dioxide, and the oxides of nitrogen, were studied by Y. Miyake, and the seasonal variations of their contents were also determined.

Geochemical researches in Japan were influenced equally by European and American publications in geochemistry. *The Data of Geochemistry* of Clarke and reports of Gold-

schmidt are well known in Japan, and the *Géochimie* of Vernadsky was translated into Japanese in 1933.

Geochemical research in Japan has been prosecuted mainly by chemists rather than geologists or mineralogists, and this is probably one of the reasons why the lithosphere is less investigated than the hydrosphere.

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A FLORIDA LANDSLIDE[1]

RICHARD H. JORDAN
Florida State University

During the first week in April, 1948, a landslide carried about 4 acres of a farm owned by W. D. Pitts of Greensboro, Florida, into an adjacent valley. The magnitude of the landslide, as well as certain other of its features, makes it worthy of note.

Greensboro is situated in northwestern Florida on a low plateau about 290 feet above sea level. The plateau is underlain by horizontal strata, probably of Miocene age. Regional youth is shown by flat-topped interfluves, but the larger valleys are mature or old. Because the larger valleys are cut less than several hundred feet into the partially consolidated sediments, the available relief is less than the critical value (Glock, 1932). It follows, therefore, that the interfluves may well go from youth to old age without divide maturity through lateral planation by the larger streams.

The landslide moved about 4 acres of land lying on the gentle slope (not exceeding 10°) of a wide spur extending into a swampy east-west valley. The ground had been thoroughly saturated by a 16-inch rainfall during the 30 days preceding the slide. This rain marked the culmination of a record 12-month fall, so that the region was in flood condition. For the 12-month period ending March 31, 1948, 89 inches of rain had fallen, against an average of 55 inches.[2]

Approximately 146,000 cubic yards of reddish, partially indurated clayey sands of the Hawthorne formation broke away from the hillside, leaving a horseshoe-shaped scar and a cliff of similar shape about 45 feet in height (pl. 1). The material funneled through the narrow opening of the horseshoe over a low lip into the marshy valley bottom. Because of the damming effect of the dense forest, the material did not cross the valley but spread out up and down the valley for several hundred yards, much as wet cement flows when poured from a mixer onto a flat surface. The floor of the scar is almost on a level with the valley bottom and shows very little slope.

The land composing the upper part of the

[1] Manuscript received August 30, 1948.

[2] The rainfall for the 3 months of 1948 was 23 inches, against an average fall of 12.50 inches for these months.

Aerial view of slide, from the north

landslide was a cultivated field, whereas the lower part was forested with tall pines and other trees. The swampy valley was likewise heavily wooded. The flow demolished and buried the wooded area of the swamp and overturned the large pines on the hillside. Trunks and other parts of the trees may be observed at all angles in some parts of the debris, although most of the forest was completely buried.

According to Sharpe's classification (1938), the Greensboro landslide is primarily an earthflow of the more rapid type, with slumping around the upper margins. The general shape, including the bottleneck, is somewhat comparable to the 1898 flow near St. Thuribe, Quebec, although in rate of movement it is assumed that the Greensboro flow was more rapid (Sharpe, p. 51). There was no eyewitness, but the general appearance indicates completion of movement in minutes rather than in hours.

The sediments involved are not completely indurated, and there seems to be no definite indication of a clayey or otherwise slippery plane surface on which the material slid as a whole. However, the Hawthorne formation has many lenses of fairly pure clay, which would greatly facilitate the flowage of saturated sediments. Furthermore, the sand of the formation is commonly admixed with varying amounts of clay.

After the flooded streams in the vicinity subsided, several small ponds fed by springs remained at the bottom of the scar, but no springs appeared in the walls of the cliff. Thus it seems probable that the water table at the time of the flow had been raised high enough to saturate thoroughly the otherwise comparatively well-drained layers forming the bottom of the flow. The flowage of this saturated zone may well have instigated the landslide.

Echelon mounding of the landslide topography indicates that the mass did not move as a whole but that the lower parts broke away as an earthflow, to be followed immediately and in turn by higher segments, each segment slumping toward the center of the amphitheater formed and thus enlarging its horseshoe shape. This shape is more easily accounted for by such movements than by a simultaneous slipping of the whole mass.

When all sections of the land had broken away and were well on their way to the valley, the early deposits began to dam the flowage of debris in the rear. This is indicated by the general aspect of the floor of the amphitheater, the floor being somewhat lower near the cliff than in the middle of the bowl and sloping gently from this point toward the valley floor.

Probably the most unusual feature of the whole flow is the low angle of the slope of the land previous to the slide, the maximum slope not exceeding 10°. Thus it is evident that the floor of the amphitheater, 45 feet below the top of the cliff, slopes very gently toward the valley and that the material which moved on the bottom moved on a very slight slope indeed.

Considering the high annual rainfall of this section of Florida and the semiconsolidated nature of the sediments, it might be reasoned that flows of this type and magnitude would not be unusual. A few somewhat similar small landslides more closely resembling slumps and at least one large incipient landslide in which only the cracks have formed have been noted in Florida this year. However, no actual landslip has been seen which is comparable in magnitude to that at Greensboro.

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DISCUSSION

THE DISTRIBUTION OF OXYGEN IN THE LITHOSPHERE AND OXYGEN IN ROCKS: A BASIS FOR PETROGRAPHIC CALCULATIONS

A DISCUSSION[1]

IVAN TH. ROSENQVIST
Norwegian Defence Research Establishment

Tom F. W. Barth (1948*a*, *b*) has discussed the distribution of oxygen in the lithosphere and outlined a new method of petrographic calculation. The latter subject was also discussed by Barth in another paper (1947).

Barth's paper on the distribution of oxygen in the lithosphere contains a restatement of what Barth calls "Ramberg's principle." "Ramberg's principle" has been discussed in another paper (1947), where I raised objections against the applicability. The chief objection is that the chemical activity of a crystalline substance is dependent not only on the temperature and pressure but also on the surrounding phases.

It is well known to all chemists working with reactions in the solid state that traces of alien ions in a crystal lattice have considerable influence on the activity of the crystal. It is generally unknown how the solubilities of different substances in different crystal lattices are related to pressure. Nothing can be stated, therefore, as to the variation of the chemical potential of a mineral component with pressure. "Ramberg's principle" is valid only in a pure system in a vacuum. In the earth the reactions may develop just opposite to the direction to be expected according to this principle.

Barth's calculations of the stability ranges of the different iron oxides would therefore, at best, be valid only for a globe consisting solely of oxygen and iron and cannot be directly applied when conditions in the earth are discussed.

At this point it is worth mentioning that the transition: $4Fe_2O_3 + Fe^{2+} = 3Fe_3O_4 - 2e$ involves a considerable increase in volume as follows:

4 hematites represent a cell volume = 204 Å^3,

3 magnetites represent a cell volume = 225 Å^3.

It can hardly be correct to calculate the distribution of iron oxides in a gravitational field without taking this fact into consideration. Barth's assumption might easily come in opposition to the Le Chatelier principle.

That an increase in pressure actually can break chemical bonds is well known to chemists. Brown and Patterson (1948) have dealt with a somewhat similar problem. They are, however, very careful in their conclusions and state (p. 92) that "although existing data, such as shown in figure 2, support such an assumption, definite proof must await refined thermochemical calculations, more precise measurements of distribution coefficients, and broader theoretical foundation of the effect of high pressure on partial molar volumes."

Barth knows that he is dealing with rather hypothetical and idealized conditions. In the last half of his paper, therefore, he deals with the applicability of his calculations. He finds that in some cases, e.g., the distribution of uranium, chemical forces make "Ramberg's principle" invalid. In the case of oxygen, however, he does not think that the chemical affinities are of the same importance. On the basis of oxygen content of olivine, basalts, "average igneous rock," and "ichor granite," he concludes that the oxygen in the lithosphere shows a tendency to be distributed according to to "Ramberg's principle."

If his table 1 (p. 42) is more closely considered, it is seen that the oxygen content rises about 1.1 per cent in going from basalts to "ichor granite"; at the same time the silicon content rises 31.5 per cent. As quartz contains almost 99 per cent oxygen by volume, an increase in the quartz (and feldspar) content of a rock will naturally involve an increase of the

[1] Manuscript received September 29, 1948.

relative oxygen content. As the rocks rich in quartz (and feldspar) are lighter than the average lithosphere, it is not necessary to assume any diffusion process or any "Ramberg's principle" to explain the fact that the rocks of the upper sial are richer in oxygen than are the rocks of the deeper strata. It is not necessary to assume any diffusion or any "Ramberg's principle" to explain why the upper part of a milk bottle is richer in fat than the lower part.

As stated by Bowen (1948), Niggli and Brandenberger (1948), and myself (1947), the diffusion coefficients of ions in solid mineral lattices are far too low to cause any transport of matter worth mentioning if the distance is of the order of 10–100 km. Even diffusion coefficients as high as 10^{-4} cm²/sec (as in water) are too low to permit diffusion of any importance in one million years. The diffusion coefficient in mineral lattices are by no means as high as 10^{-4} cm²/sec even at an elevated temperature. Most probably the right value in the deep, but still crystalline, part of the lithosphere is about 10^{-15} cm²/sec. Bowen (1948) gives diffusion coefficients of about 10^{-10} at 1,400° C. in rock-forming minerals. In uraninites I have found (1949) coefficients of $2–4 \cdot 10^{-22}$, in uranothorites $1 \cdot 10^{-18}$, and in samarskites $4 \cdot 10^{-19}$. These values are the average diffusion coefficients of lead since the crystallization of the minerals.

For example, we may assume that the diffusion coefficient of an element in the rock-forming minerals is as high as 10^{-12} cm²/sec (a value probably very much too high). Let the rock in question contain 0.1 gm. of the element in question per cubic centimeter, and let the activity of the element be doubled by raising the pressure 300 atm. (equivalent to 1,000 meters of overlying rock). (The pressure increase necessary to double the activity will probably by much higher.) These assumptions give an active gradient of the order of 10^{-6} gm/cm⁴. If the diffusion coefficient is 10^{-12} cm²/sec, 10^{-18} of the element in question will diffuse through a cross-section of 1 cm² in 1 sec., i.e., 0.03 mg. in a million years. Such a diffusion is without any geological significance.

Barth's table 1 (p. 42), showing the distribution of oxygen in the lithosphere with various depths, is a statement of fact. It is not, however, related to the degree of oxidation of the iron, as might be concluded from Barth's paper.

According to Clarke and Goldschmidt (course in petrology at Oslo University), the FeO/Fe_2O_3 ratios in weight percentage of different rocks are as shown in the accompanying tabulation. These values are all higher than the value for magnetite (0.45). It is obvious that the escaping tendency of oxygen in a rock is dependent upon how the oxygen is chemically combined, e.g., the oxidation state of the iron. The volume percentage of oxygen in a rock is an indicator of the escaping tendency, *only as long as all the cations occur in the same amount.* Thus the escaping tendency of oxygen in quartz with about 99 per cent by volume of oxygen is not necessarily greater than in K_2O with about 30 per cent by volume of oxygen. A mere statement of the volume percentage of oxygen in rocks at various depths, therefore, seems irrelevant to a discussion of the chemical potential of oxygen in the different rocks.

	Av.
Igneous rocks	1.23
Peridotites	1.73
Anorthosites	1.06
Basalts	1.18
Gabbros	1.93
Granodiorites	1.62
Granites	1.00
Keratophyres	1.75

The theoretical treatment of "Ramberg's principle" is based on the free passage of oxygen upward. Barth states (1948*a*, p. 43) that the higher oxides of iron are unstable at greater depths, where "they dissociate, with formation of oxygen, which also migrates upward." However, elsewhere Barth says that the oxygen might be regarded as a glassy solvent, in which "Ramberg's equilibrium" is established by migration of cations.

Barth seems to think that all rocks containing equal quantities of oxygen will take up the same volume. This assumption is obviously a misconception. He states that the volume relations of ordinary rocks are almost wholly determined by the oxygen ions. The most important factor in the volume relation of the rocks is, however, the packing index of the minerals. H. W. Fairbairn (1943) has handled this in an excellent way.

We now come to the subject treated in Barth's second paper, namely, the new method for petrographic calculations. In short, the method is based on the following: All ordinary rocks have a volume ratio of oxygen/cations = 92/8. Barth says, therefore, that, if one wants to compare rocks of equal volume, one can compare rocks containing the same amount of oxy-

gen ions. As a standard comparison unit it is expedient to choose a volume comprising 160 oxygens, for in an ordinary rock nearly 100 cations are associated with 160 oxygens. Barth calls a rock unit containing exactly 160 oxygen ions the "standard cell."

It is evident that different minerals containing the same amount of oxygen, e.g., 160 oxygen atoms, may have very different volumes. This is valid even for minerals in the same metamorphic facies; comparing two minerals in the sanidinite facies—corundum, for example, with a "standard cell volume" of 160 oxygen atoms = 2,300 Å^3, cristobalite with a "standard cell volume" of 160 oxygen atoms = 3,520 Å^3. Barth says (p. 56) that some petrologists introduce entirely arbitrary assumptions, such as iron or alumina being constant during metasomatism. He himself assumes that the oxygen content is constant during the metasomatism. This assumption is, of course, also arbitrary. Barth thinks in this way to attain a method automatically satisfying the volume relations. This must be a mistake. We may, for example, consider the metasomatic transformation of dolomite into diopside. According to Barth's view, this process takes place by diffusion of Si^{4+} ions into the carbonate rock, followed by a replacement of the carbon in the carbonate and a diffusion of C^{4+} out of the rock. This leads to volume contraction of about 5 per cent.

In other cases, such as the transformation of spinel into olivine by replacement, the effective volume expansion would be nearly 20 per cent if the amount of oxygen is kept constant.

Barth, several times, writes carbon ions and silicon ions. It is, however, doubtful whether the four valent positive ions of carbon and silicon have any probability of existence in any terrestrial system. The C^{4+} ions have never yet been found, except at solar conditions. The ionization energies are extremely high in a system containing oxygen.

Barth's theories dealing with the inorganic cycle of oxygen do not seem to be in harmony with the rest of his paper. Here he pleads for the stationary oxygen "solvent." It is difficult to understand why the oxygen should be stationary because of its great ion radius (= 1.32 Å) and why potassium (ion radius = 1.40 Å) should be very mobile.

In any case, the diffusion coefficients in rocks are too low to cause any mass transport in the solid state over a great distance. The whole question is therefore a purely academic one.

However, I am not of the opinion that diffusion in the solid state is unimportant in metasomatic processes. In another paper I have compared metasomatism with the metabolism of living creatures, e.g., the oxygen flows with blood through arteries and capillaries, it diffuses through the cell walls into the cells. The excreta diffuses from the cells into the capillaries and flows to the kidneys and lungs.

Twenty-five years ago, Manjero Watanabé, in an excellent discussion of the diffusion process in geology, was aware of the impossibility of a long-distance diffusion in the earth. He writes (p. 134):

> Diffusion can perform metasomatism through fine passages even where circulation is difficult, while circulation furnishes the new material and removes the old substance and thus maintains the concentration necessary for diffusion. Without circulation, diffusion would be unable to accomplish much, because of its small velocity and of the exhaustion of material in the nearby source. Without diffusion, circulation would be unable to replace the mineral, particle by particle; it can only dissolve the material on the surface of relatively large fissures and there deposits a new substance, but not beyond this zone.

In closing, I would mention that sulfide minerals commonly seem to have replaced oxide minerals without volume change. In these cases it is obvious that the oxygen has moved.

I am aware of the fact that this paper has been an entirely negative one, that I have not suggested any better method of solving the problems dealt with by Barth. Elsewhere, I have in some manner dealt with similar problems, and, until a better solution can be found, I prefer the more conservative interpretations, although I am aware of their possible shortcomings.

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THE DISTRIBUTION OF OXYGEN IN THE LITHOSPHERE: A REPLY[1]

TOM. F. W. BARTH
Universitetet i Oslo

In the preceding discussion Mr. I. Th. Rosenqvist is not so much concerned with the distribution of oxygen in the lithosphere as he is opposed to "Ramberg's principle." This principle was demonstrated some sixty years ago by Willard Gibbs; now Rosenqvist is opposed to its being applied to the stability relations in the crust of the earth as was recently done by Mr. Ramberg.

In an attempt fully to understand Mr. Rosenqvist's standpoint I have carefully read his previous discussions (1947 and 1948). As far as I can make out, Rosenqvist's final, general complaint can now be reduced to what is contained in the following passages: ". . . the chemical activity of a crystalline substance is dependent not only on the temperature and pressure but also on the surrounding phases . . . traces of alien ions in a crystal lattice have considerable influence on the activity. . . . Nothing can be stated, therefore, as to the variation of the chemical potential of a mineral compound with pressure. . . . Ramberg's principle is valid only in a pure system in a vacuum."

Let us see how thermodynamics gives us the relation between chemical activity and pressure in a rock. To this end let us consider a volume unit of a rock over a range of temperature and pressure. The component phases of the rock are the various minerals and, at higher temperature, a "pore" liquid (or gas); these phases may contain traces of any of the existing chemical elements. At a given P-T condition we assume the phases to be at equilibrium, and the chemical activity of, say, oxygen is therefore the same in all phases. Suppose the rock contains quartz, as many rocks do. With increasing pressure the chemical activity, a, of oxygen in quartz will increase from a_0 to a_P according to the equation

$$a_P = a_0 e^{\Delta PV/RT}, \qquad (1)$$

where ΔP is the increase in pressure and V is the fictive volume of oxygen in quartz.

In all other component phases of the rock the activity of oxygen will increase according to the same formula (1). Since the fictive molal volume of oxygen may be somewhat different in the different phases, the activity of oxygen will not be exactly the same for all phases at the new pressure, P. Reactions will therefore take place in such a way that the small oxygen activity differences are eliminated and the new stable oxygen activity will be of a value intermediate between those given by formula (1) when applied to the components with the greatest and smallest V.

It seems to me that Rosenqvist, instead of considering the effect of the pressure, has discussed the effect of the addition of a reactive solvent to the rock powder. Now the "pore solution" may be regarded as a solvent, but it is hardly very reactive; and the claim that it may make "the reactions develop just opposite to the direction to be expected" will be received, I believe, with great skepticism by most petrologists.

Incidentally, similar thermodynamic equations have subsequently been applied to petrogenetic processes by Brown and Patterson (1948), who won the prize of the American Association for the Advancement of Science for their paper and whom Rosenqvist quotes with approval because they state that definite proof must await refined thermodynamical calcula-

[1] Manuscript received March 3, 1949.

tions. But if Rosenqvist's assertion is true, that "nothing can be stated as to the variation of the chemical potential of a mineral compound with pressure," then it seems to me that the refined thermodynamical calculations are of no use.

To be sure, Rosenqvist's objections are not new to petrologists. In his latest textbook Niggli states that, although theoretical chemistry is based on ideal conditions which are never attained in nature, it would be quite wrong to conclude that it has no application. Niggli (1948, p. 375) says:

> Da der Mensch, eingespannt in das von ihm geschaffene Begriffsystem, gerne zur Einseitigkeit neigt, hat man under Bezugnahme auf die erwähnten Erscheinungen auch von einer Nictanwendbarkeit der Phasenlehre auf die Gesteinskunde gesprochen.
>
> Die Theorie geht von idealisierten Verhältnissen aus, die gar nicht erreichbar sind. Aber es wäre naturgemäss unrichtig, aus diesen Widersprüchen zu schliessen, die klassische Phasenlehre sei überhaupt nicht anwendbar oder es sei, ausgehend von den gleichen Prinzipien, unmöglich, sie anderen Voraussetzungen anzupassen. Hat er das Geschehen in grossgeologischen Räumen zu überblicken, so muss er zu den thermodynamischen Grundprinzipien zurückkehren.

Throughout his discussion Rosenqvist is concerned with questions of no pertinence to the article which he professes to criticize.

For example, he brings up the volume relations in the equation:

$$4Fe_2O_3 + Fe^{++} = 3Fe_3O_4 - 2e\ .$$

But this equation should not be discussed without considering the relations of FeO, and in my article I made special mention of the fact that "the possible effect of the 'reversed' specific gravity relations of hematite and magnetite will not be discussed here." Now an example involving the same problem has been treated by Ramberg (gypsum $\rightleftarrows$ anhydrite + water), and Rosenqvist may be referred to this discussion (1949*b*).

Likewise, the quixotic fight put up by Rosenqvist to prove that "[diffusion through crystalline minerals] is without any geological significance" falls outside the scope of the article discussed by him. In my article I made no assumption as to the mechanism of the migration of the atoms, and Ramberg (1949*a*) says that he "considers it unlikely that large-scale diffusion through solid rocks preferably takes place through the silicate lattices."

Rosenqvist misrepresents my view by assuming that "oxygen should be stationary because of its great ionic radius, = 1.32 Å, [whereas] potassium, ionic radius = 1.40 Å, should be very mobile." My statement reads (p. 47): "Owing to the great bulk of the oxygen ions, they probably furnish a rather stationary medium." In this sentence I referred to the total bulk of the oxygen ions in the lithosphere (=92 per cent by volume), not to the ionic radius. I cannot remember having said that potassium is "very mobile," and I have certainly as yet never suggested any general relation between mobility and atomic radius.

The listing of the cations associated with 160 oxygens, as required by my scheme of petrographic calculations, does not imply that the cations necessarily have moved as such; nor does the listing of rock analyses in terms of oxides imply that the oxides exist as such. Ramberg says (1949*a*, p. 52):

> It can be shown that the mobility of the several particles in rocks increases with rising partial vapor tension of the particles. It seems very probable that large-scale migration through solid rocks takes place with the help of "gaseous" atoms and molecules adhered to the mineral surfaces or the mosaic fissures.

To me the picture of the mechanism of the diffusion is still uncertain and tentative. But the solution may perhaps now be found in the highly interesting structural control of the metasomatic processes as recently demonstrated by Newhouse (1949).

At this point it should be emphasized that thermodynamics tells nothing about the velocity and mechanism of a reaction. Thermodynamics gives us only the directions of the several reactions. The actual reactions may be slow or fast and may use different paths, but each reaction that takes place brings the system of rocks closer to equilibrium.

There are and will always be conservative geologists, but thermodynamics favors neither conservatives nor progressives; it simply presents us with the ideal equilibrium conditions; and observations in nature, not in theory, must decide whether or not equilibrium has been attained.

Rosenqvist favors what he calls a "conservative interpretation" and thinks that the upper part of the lithosphere is rich in quartz and feldspar for the same reason that the upper part of a milk bottle is richer in fat than the lower part. Now many serious students of the subject

may look upon this as a very radical interpretation indeed. But in thermodynamics these words have no meaning. Thermodynamics simply states, as I have endeavored to show in my article, that equilibrium in the lithosphere is attained if oxygen accumulates near the top. And observations have shown that in the earth's crust oxygen consistently decreases with depth.

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THE DISTRIBUTION OF OXYGEN IN THE LITHOSPHERE COMMENTS ON BARTH'S REPLY[1]

IVAN TH. ROSENQVIST

Norwegian Defence Research Establishment

I may summarize my objections as follows:

1. I do not think that it is correct to apply the equations of Gibbs in a complex system (i.e., in a rock).

2. "As long as it is unknown how the solubility of different substances in different crystal lattices are related to pressure, nothing can be stated . . . as to the variation of the chemical potential of a mineral compound with pressure." Consequently, refined thermodynamical calculations and laboratory investigations are necessary before anything can be established as to the variation of the chemical potential with pressure.

3. When Barth prefers to term my discussion of the unimportance of diffusion "quixotic," it must be remembered that diffusion (through the crystals or through the intergranular film) is the *only* path of transport where an investigation of the variations of chemical potential is of any interest.

THE USE OF OXYGEN AS A REFERENCE SUBSTANCE IN PETROGRAPHIC CALCULATIONS: A SUPPLEMENT AND A REPLY[2]

TOM. F. W. BARTH

Universitetet i Oslo

In recent years much attention has been given to the problem of granitization and, generally, to metasomatic material transfer in rocks and reactions in the solid state. Elsewhere I have reviewed some of the recent contributions to this subject (Barth, 1948*b*). But nobody, I believe, has given credit to the first French papers that actually initiated this school of thought fifteen years ago.

Perrin (1934), extrapolating from metallurgical observations, formulated the hypothesis that metamorphism (metasomatism) was due to diffusion in the solid state in opposition to the existing theories that called for intervention of liquids or gases. Likewise, he suggested (1935) that granite was the product of such diffusions and had never crystallized from a liquid magma. In regard to material exchange in solid rocks

[1] Manuscript received March 14, 1949.

[2] Manuscript received March 3, 1949.

Perrin writes (personal communication 1948, translated from French):

> The first question is to find out if the atoms of certain chemical species remain in place. Lapadu-Hargues [1945] in his study of metamorphism first thought that silicon and aluminium did so. We [Perrin and Roubault, 1946] pointed out to him that his own statistical methods demonstrated an appreciable variation in the ratio Si/Al and that nothing indicated that any of these species remained stationary. In the meantime you [Barth, 1948*a*] reached the conclusion that the solvent ions to a large extent were stationary and that the cations moved through this solvent. A priori this seemed a probable hypothesis to me in view of the enormous preponderance of oxygen in the lithosphere. . . . Subsequently, Lapadu-Hargues [1948] came to the same conclusion. On a small scale, however, some oxygen diffusion may occur, e.g, in the case of pyrite replacing oxides, etc. . . . It seems to me that all one may say is that, because of its very great concentration, oxygen is without doubt the atom that diffuses the least and plays the role of a solvent. But it does not mean that the frame of oxygen remains unchangeable, the shape of the frame certainly changes. Perhaps you have generalized too much in saying that the volume remains constant during metasomatic exchange. No doubt there are cases where observations would seem to demonstrate that the changes in volume have been zero or insignificant. But Brajnikov [1945] has pointed to the variation in the number of oxygen atoms per cubic centimeter of the different minerals. The result is that if, e.g., a crystal of orthoclase during granitization devours a crystal of hornblende or quartz, then the oxygen lattice will expand; but if quartz replaces a plagioclase the lattice will contract. . . . What takes place on a small scale may take place on a large scale. Brajnikov directs attention to the fact that, although granular rocks show approximately the same value for the total concentration of oxygen per cubic centimeter, the corresponding value is larger for detritic rocks. . . . It seems to me as an automatic conclusion, if no notable diffusion of oxygen has occurred, that metasomatism and granitization are generally accompanied by a total dilatation of the oxygen lattice and by an increase of the volume of the rock. This is a verification of the hypothesis of metamorphism as a generator of folding as I announced in 1935.

Some of the above conclusions of Mr. Perrin will be discussed presently. First, I want to quote some statements by Rosenqvist in a preceding article in this *Journal* (p. 422): "This assumption [that the oxygen content is stationary during the metasomatism] is, of course, also arbitrary. Barth thinks in this way to attain a method automatically satisfying the volume relations. This must be a mistake."

In this connection reference should be made to Brajnikov's article (1945), in which he has calculated the absolute amount of oxygen in the various minerals and rocks expressed in terms of the number of oxygen ions contained in a cubic centimeter of the substance. I have shown (Barth, 1948*c*) that Brajnikov's article supports my point of view. I shall repeat the salient points here.

As can be seen from Brajnikov's tables, gabbro and granite have very nearly the same number of oxygen ions per cubic centimeter. Many of the ordinary rock types are transitional between gabbro and granite and therefore also contain the same number of oxygen. Granitization of gabbroic and, generally speaking, of metamorphic rocks takes place, therefore, without appreciable change in volume if the oxygen content is kept constant. This is a fact and no mistake.

It should be remembered that in previous papers I have repeatedly emphasized that only in the ordinary silicate types of rock belonging to the same mineral facies are the volume relations, in this manner and in their first approximation, determined by the oxygen ions. It is therefore completely irrelevant when Rosenqvist discusses the volume relations of corundum and cristobalite.

Perrin's discussion raises a different problem. He thinks that complete granitization of sedimentary rocks involves a volume expansion of about 8 per cent, and he remarks that this corresponds to a linear expansion of only 2 per cent, a rather small amount, which explains the *mise en place* of many granites showing no derangement of the adjacent sedimentary strata. On the other hand, if one looks at large-scale granitizations comprising great volumes of the crust of the earth, like the roots of mountain chains, then the effect of expansion becomes of importance—indeed, Perrin thinks that it may be a factor in orogenesis.

I agree with Perrin that, particularly if transitions from one mineral facies into another takes place, for example, from the facies of sedimentary rocks into that of a granite, then the volume differences become of importance. (But an increase of 8 per cent is in my opinion not well supported.) It may be recalled that, for example, the transition from eclogite facies into amphibolite facies includes the important reaction

jadeite + omphasite $\rightleftarrows$ plagioclase + hornblende,

which is accompanied by a volume increase of 15–20 per cent.

Finally, I should like to clarify the meaning of a "stationary" oxygen medium. Let us specifically consider the granitization process: (1) One cubic centimeter of the educt has approximately the same number of oxygen ions as 1 cubic centimeter of the product. (2) The metasomatic process has taken place volume by volume. (The volume law may be questioned, but from many places of the world it has been shown to hold, and from nowhere do we find decisive evidence to the contrary.) Consequently, we need not *in our calculations* consider any migration of oxygen; therefore, we may say that oxygen is stationary. But if, for a brief moment, we look into the actual mechanism, the picture becomes different. Thermodynamic equilibrium at a phase boundary does not mean a state of no motion, it means that an equal number of atoms or molecules migrate both ways, the net result of the transport being zero. Oxygen ions may move about in the rocks, but, following no preferred direction, they move statistically, usually causing no noticeable concentration in any part of the rock. They may act as carriers for silicon or aluminium, however; for the strongly bound silicon ion, incarcerated, as it were, in the center of an oxygen tetrahedron, may not easily break loose without the accompaniment of one or more oxygens. But the net result is that silicon moves, while the oxygen content remains "stationary."

Therefore, I do believe that the use of oxygen as a reference substance in petrographic calculations is justified and may prove fruitful in the study of, e.g., granitization, basic fronts, and similar phenomena.

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THE PRESERVATION OF ANTARCTIC ICE SPECIMENS: A DISCUSSION[1]

HENRI BADER

Rutgers University

The purpose of A. D. Howard's paper, "The Preservation of Antarctic Ice Specimens," which appeared in the January, 1948, issue of the *Journal of Geology*, is to demonstrate the feasibility of large spatial and temporal separation of glaciological field work and laboratory investigation of collected ice specimens.

Howard's thesis is largely fallacious because of the fact that the structure of ice aggregates is very sensitive to environmental change, particularly to variation of temperature and stress, and probably also to hydrostatic pressure. I have observed great changes of grain size due to recrystallization in thin sections of ice stored for no more than 1 or 2 weeks at $-5°$ to $-10°$ C. This ice was fine grained (2–3 mm.) and had been subjected to stress prior to sectioning. Recrystallization is slower, the larger the grain size, the smaller the stress prior to sectioning, and the lower the storage temperature.

Thus the structure of ice specimens taken from stressed portions of a glacier, from drill

[1] Manuscript received January 31, 1949.

cores or crevasses, should be examined soon after collection or stored at very low temperatures, for example, on dry ice (CO_2). Strong cooling, however, tends to crack larger specimens, rendering them useless for ice-mechanics work.

Wet ice can be frozen for sectioning, but it is questionable whether it may be frozen for subsequent mechanical testing. It remains to be proved that freezing and rethawing does not significantly upset a phase equilibrium which has been established during many years at pressure melting temperature.

The high sensitivity of ice structures to external conditions promises to be a valuable source of information on their near and remote past history. Addition of the impress of storage tends to obliterate the picture of at least the near past physical history. If laboratory work is to be done on natural ice, laboratory facilities should be provided very near to the source of the ice to be examined.

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THE PRESERVATION OF ANTARCTIC ICE SPECIMENS: A REPLY[1]

ARTHUR DAVID HOWARD
Stanford University

The best procedure for studies of natural ice is on-the-spot investigation by specialists with all necessary equipment and with adequate time at their disposal. Unfortunately, this is impossible on Antarctic expeditions unless the studies are conducted in the immediate vicinity of base camp. Travel farther afield generally involves arduous sledge journeys, in which the problem of weight necessarily restricts the amount of equipment that can be taken along. The airplane and tracked vehicle may eventually eliminate this restriction. Unless the ideal combination can be arranged, however, and until we understand better the situations in which changes occur and the magnitude of the changes involved when ice specimens are removed from their natural habitat, it seems advisable to encourage wide sampling, even if the detailed examinations must be made later.

Dr. Bader opposes any delay in examination of ice specimens because "the structure of ice aggregates is very sensitive to environmental change. . . ." His entire criticism is based on the observation that "great changes of grain size due to recrystallization" took place within 1 or 2 weeks in thin sections of previously stressed ice which had been stored at $-5°$ to $-10°$ C. One might conclude, therefore, that ice removed from a stress environment changes so rapidly, regardless of temperature control, that immediate examination is mandatory. Yet Bader says that when specimens from stressed portions of a glacier cannot be examined soon after collection, they should be "stored at very low temperature." Thus Bader himself seems to concede that delay is feasible, if proper temperature precautions are taken. In view of this concession, I find it difficult to understand his strong condemnation of my proposal.

Recrystallization involves molecular transfer. Transfer in both liquid and vapor phase falls off with the temperature. For example, the vapor pressure of ice at $-30°$ C. is less than seven one-hundredths of that at $0°$ C., and at $-40°$ C., only a little over two one-hundredths. Storage at low temperatures, therefore, should retard very appreciably any changes due to recrystallization. Nor is it likely that there are significant changes other than those due to recrystallization. Ice under stress, as in a glacier, does not rebound to its original form when the stress is removed, nor do grains deformed by crystal gliding spring back to their original shape. The stress characteristics remain in the sample and may be modified only by recrystallization. But if storage at very low temperature can vastly retard recrystallization, then no unnecessary haste in examination of properly stored specimens is required. The effectiveness of low temperature in retarding increase in grain

[1] Manuscript received February 14, 1949.

size is suggested by the observations of Wright and Priestly (1922, p. 124), who found "no tendency toward an increase in mean size of glacier grain at high pressures (great depth) in Antarctic glaciers."

One gathers from Bader's observations of change in grain size in sections stored at $-5°$ to $-10°$ C. that the temperatures were not low enough or that additional precautions against recrystallization were needed. Seligman (1948, p. 191) cites the interesting experiments of De Quervain, in which delicate snow crystals, immersed in paraffin oil at a temperature of $-10°$ C. showed no alteration after periods of months. Alteration did take place, however, at $-5°$ C. Demorest (1940, p. 127) stored specimens for months in kerosene at low temperatures but unfortunately did not publish on the results.

The specimens which I brought back from the Antarctic included not only samples of "stressed" ice, but also of névé and sublimation crystals. With regard to specimens other than glacial ice, a few observations by Wright and Priestley seem pertinent. They found (1922, pp. 79, 108) that certain megascopic structures in ice actually became more pronounced—therefore more easily studied—when the samples were subjected to ablation for periods up to 2 weeks in temperatures slightly below freezing. They found, too (p. 103), that, in contrast with the situation in temperate climates, changes in surface névé in Antarctica required a considerable lapse of time. They report that, after 2 years, at a depth of about 2 feet in the Ross Barrier, the individual grains still preserved the original subangular polygonal form. Such observations would seem to suggest that, if specimens of névé were stored under proper temperature conditions, the original characteristics might remain unaltered for considerable lengths of time.

None of the above comments are to be construed as arguments against research on possible changes in natural ice when it is removed from its habitat. There is also need, however, for additional research on improved storage techniques to insure a minimum of change in ice specimens which cannot be examined immediately.

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ALBITE–WATER SYSTEM IN THE GRAVITATIONAL FIELD: A COMMENT[1]

HANS RAMBERG
University of Chicago

The effect of the gravitational field on chemical equilibrium is usually negligible in laboratories, and the subject, therefore, is little discussed in the physicochemical literature. In petrology and geochemistry, however, any effect of the gravitational field—one of the principal properties of the earth—should be considered. With that in mind, the writer has emphasized the influence of gravity upon large-scale chemical equilibrium in the upper shells of the earth (1946–1949); later Barth dealt with the same phenomenon (1948), as did Verhoogen in his interesting paper on the "Thermodynamics of a Magmatic Gas Phase" (1949*b*) and in a discussion (1949*a*).

Verhoogen derives the thermodynamically correct relation between composition of a solution and height in the gravitational field (eq. V13, p. 129, 1949*b*) and applies this equation to the hydrous albite melt investigated by Goranson (1938).

At 1,000 bars and 900° C. an albite melt will dissolve 6 per cent water (provided that the partial water pressure corresponds exactly to the total pressure). From the data available and equation V13, Verhoogen finds that at 2,900 meters higher in the column of solution the concentration of water will be 8.2 per cent, because

[1] Manuscript received February 28, 1949.

of gravitative differentiation. At this higher level the hydrostatic pressure in the solution is 500 bars, and Goranson's experiments show that only 4 per cent of water can dissolve in the melt. Verhoogen therefore arrives at the following conclusions (1949*b*, p. 130):

> The effect of the gravitational field is thus to increase very noticeably the saturation concentration. . . . In the same manner, a saturated solution at 900° C. and 500 bars (4 per cent) would be in equilibrium with a solution located at 2,900 m below and containing only 2.8 per cent of water instead of 6 per cent. . . . It follows that if the effect of the gravitational field were to disappear suddenly, as if, for instance, the upper and lower halves of the magma column were brought mechanically to a common mean level, the upper half would be strongly supersaturated and the lower half strongly undersaturated with respect to the new position.

These conclusions, if correct, would indeed be of the greatest geological importance; for it is only necessary to provide a sufficiently thick layer—or high column—of silicate melt–water solution in the earth and the upper part would easily dissolve 20, 30, or 50 per cent of water without any supersaturation. As such water-rich silicate melts must have a low solidification temperature, we could easily explain the formation of many granites and pegmatites which now are very problematic.

Imagine, now, that we follow the principle of Verhoogen and choose an albite melt with 6 per cent of water at 900° C. and 1,000 bars. According to Goranson, this system will be a homogeneous melt. Going deeper and deeper down in a column of such a solution, the concentration of water should decrease according to equation V13 (Verhoogen, 1949*b*), and at a certain depth the solution will contain, say, only 1 per cent of water. It is well known from Goranson's experiments that such a solution is strongly unstable and will crystallize to albite under expulsion of water (the same is true for the melt with 2.8 per cent of water). According to Verhoogen's conclusions, however, this rather "dry" albite melt will be a stable liquid at as low a temperature as 900° C. because it is situated at the bottom of the solution column. It may be tempting to believe that many deep-seated rocks are formed from such "dry" but low-melting magmas. Before we do so, however, we should study the reliability of the conclusions a little further.

To be sure, it is not necessary to imagine that the effect of the gravitational field disappears, or to split the column into halves in order that the 8.2 per cent of water in the upper part and the 2.8 per cent in the lower part shall represent strong supersaturation and undersaturation, respectively. The 8.2 per cent of water in albite melt at 900° C. and 500 bars makes a strongly supersaturated solution, independent of any continuation with a subjacent melt in the gravitational field. Likewise, an albite melt with 2.8 per cent of water at 900° C. and 1,000 bars is strongly "undersaturated" with respect to water, entirely independent of a superincumbent column of melt.

The "supersaturated" melt will split up into melt and water-rich vapor. The "undersaturated" melt will form albite crystals and a new melt with 6 per cent of water.

When considering the effect of gravity upon such systems, one should remember that there are two equally important requirements to be fulfilled, namely, (1) equation V13 must be followed in homogeneous phases in order to insure chemical equilibrium from top to bottom in the column, and (2) the concentration of water in the albite melt cannot surpass certain limits at any level in the column, otherwise water vapor will develop or albite crystals form.

Apparently Verhoogen does not consider the second requirement; and therefore he concludes that the gravitational field alone would be able to decrease or to increase the saturation concentration as postulated.

It is easy to show that the albite melt–water column in the gravitational field at 900° C. will behave as follows: the upper part of it will expel water-rich vapor, which will gather above the top of the melt; and the lower part will yield albite crystals, gathering in a bottom layer. When complete equilibrium is attained, the upper part of the melt will just be saturated with water under the pressure there—say, 1,000 bars and 6 per cent water (if 500 bars and 4 per cent water is used, no melt will exist). The concentration of water down in the melt will follow equation V13, so that concentration of water decreases with depth. Now, since a melt with 6 per cent of water at a temperature of 900° C. is near the crystallization point (Goranson, 1938, fig. 4, p. 85), the melt column will be very short. At the bottom of that short column the concentration of water may be, say, 5.9 per

cent or just on the crystallization point. Below that level neither melt nor water vapor exists except perhaps as minute pore fillings in the albite crystalline aggregate, which is stable farther downward.

In conclusion, the gravitational field is unable to stabilize systems which are otherwise chemically unstable at the pressures and temperatures in question. The effect of the gravitational field is to change hydrostatic pressure and to change bulk chemical composition at different levels in the earth's crust.

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REVIEWS

Glaciological Research on the North Atlantic Coasts. By HANS W:SON AHLMANN. ("Royal Geog. Soc., Research Ser.," No. 1.) London, 1948. Pp. 83; figs. 44.

This publication of 83 closely printed pages, 44 figures, and numerous tables—the first volume of a new research series issued by the Royal Geographical Society—provides the most succinct and best-organized summary of H. W. Ahlmann's extensive glaciological studies yet to appear in English. Dr. Ahlmann, professor of geography at Stockholm University, has established himself as one of the world's best authorities on glacier nourishment and wastage through the researches herein summarized. His work, well known in the United States and Canada through earlier extensive writings and a 1947 lecture tour in these countries, will gain further well-deserved recognition through this publication.

Studies covering a period of twenty-eight years, 1918–1946, and dealing with six separate areas bordering the northernmost Atlantic Ocean are reported. The areas and glaciers investigated are the Styggedal and associated glaciers in the Jotunheim district of southern Norway; the Kårsa Glacier of Swedish Lapland; the ice caps of Northeast Land, the northeastern island of the Svalbard Archipelago; the ice of Isachsen's Plateau and the Fourteenth of July Glacier in West Spitsbergen; the Fröya Glacier on Clavering Island off the east coast of Greenland; and two outlet glaciers of the Vatnajökull —the Hoffell and Heinaberg—in Iceland.

This work has profited from the assistance and collaboration of a host of able colleagues best known among whom are H. U. Sverdrup and Sigurdur Thorarinsson. Ahlmann's wide acquaintance with and generous use of a great body of foreign glaciological literature, not well known to most American glaciologists, is a further asset. The subject matter is presented under the following headings.

Accumulation.—Although snow is the major source of nourishment, hail, hoarfrost, rime, and local rains that freeze before draining away are also contributors. Wind drifting of snow is favored by light snow, flat topography, low temperature, and strong wind. However, Ahlmann warns that it is a great error to think that all glaciers must have leeward orientation. Jotunheim is cited as a district which receives considerable windward nourishment in the form of hoarfrost, and on Vatnajökull the largest development of glaciers is to the windward. Recognition of the role played by freezing of rain and meltwater in nourishment, firnification, and icification will be approved by most glaciologists. Of the 485 mm. of water percolating into the snow on Isachsen's Plateau in 1934, 145 mm. changed to ice and iced firn, while 340 mm. sank through the firn and drained away. In a cold polar glacier, nearly 100 per cent of the water percolating into the firn must freeze, but its absolute volume is smaller than on temperate glaciers.

The accumulation area is that part of a glacier in which accumulation surpasses ablation. On the Fourteenth of July Glacier, 27 per cent of the total accumulation fell in the accumulation area and 73 per cent in the ablation area. By contrast, Hoffell Glacier received up to 89 per cent of its total accumulation in the accumulation area. This serves to emphasize a point repeatedly made by Ahlmann that the morphology of a glacier is of great significance in determining its regimen and behavior. One would rightly conclude from the above figures that Hoffell Glacier is the much more energetic of the two. Curves showing the amount of accumulation at various altitudes on different glaciers also display marked individuality. Accumulation usually increases with increasing altitude up to a point where it slackens off, and the rate of increase and the slack-off point for various glaciers are markedly different, depending upon climatological environment and orographic situation.

Many details of interest to students of physical glaciology, such as thermal conditions and structural features within the firn fields studied, are also summarized in this section of the article.

Ablation.—Ablation is defined as the glacier-wasting process and, in addition to melting and evaporation, is said to include calving and corrasion by the wind. The last two processes have

not usually been considered part of ablation by American glaciologists, and this definition may raise a few eyebrows. Ablation is chiefly a surface phenomenon, but it may also be internal, for wasting occurs on the walls of crevasses and in englacial or subglacial hollows and tunnels. Since meltwater may be entrapped in the glacier and refrozen, it is necessary to distinguish between gross or total ablation and net ablation, that is, the quantity of water wholly removed from the glacier by drainage, evaporation, corrasion, and calving.

The treatment of various factors and conditions controlling ablation appears to permit the following conclusions:

1. Radiation, temperature conduction, condensation, and evaporation are all factors in ablation, but radiation and temperature conduction are of primary importance, and evaporation is relatively insignificant in most environments. Condensation can be of considerable effect under proper conditions. The relative efficacy of radiation and temperature conduction depends upon many variables, including latitude, altitude, season, and climatological environment.

2. In general, the part played by radiation in causing ablation increases with latitude and altitude. Even on cloudy days in the arctic the effective radiation may be higher than on clear days. This is not true of lower latitudes and apparently comes about because of reduced albedo and the thinness of the cloud cover in far northern areas.

3. The relative influence of radiation on ablation decreases as the season progresses, and the effect of temperature increases. Late in the season when the snow line reaches its highest position and becomes the firn line, ablation is said to be largely or wholly determined by temperature conduction.

4. With decreasing altitude, temperature change and wind effects are increasingly effective in causing ablation. Detailed studies on Hoffell Glacier show that an increase in temperature has a greater absolute effect in low than in high altitude. An increase in air circulation nearly always accelerates ablation.

5. Ablation on ice is larger than on snow and by a greater degree, depending upon the importance of radiation in causing ablation. This comes about because of the greater albedo of snow.

6. The decrease of ablation with altitude is much greater on ice than on snow, 340 mm. per 100 meters as compared to 70 mm. per 100 meters in the instance of the Fourteenth of July Glacier.

7. With increasing elevation, summertime ablation accounts for an increasingly larger part of the total ablation. On Hoffell Glacier, June to August ablation accounts for 55 per cent of the total at 72 meters altitude, 60 per cent at 400 meters, and 78 per cent at 1,100 meters.

8. Gross ablation values may differ greatly from year to year. On Hoffell Glacier the 1937 ablation was only 59 per cent of that for 1936.

9. The morphology of a glacier, that is, its shape, slope, and area distribution, has a great effect on both accumulation and ablation. For example, a gently sloping ice surface permits notable shifts of the snow line with only modest meteorological change, and such glaciers are necessarily most sensitive to climatic change.

10. Meteorological conditions in the area of the snow line strongly influence the glacier regimen.

Firn and glaciation limits.—The discussion of firn limit (or line), snow line, and glaciation limit presented in this section will be of particular interest to persons not already familiar with these concepts. Glaciation limit is not defined but apparently refers to the level at which glaciers form in any particular region. It is emphasized in this treatment that the summer isotherm at the glaciation limit gives a measure, or at least an indication, of accumulation at that altitude; the higher the temperature, the greater the accumulation.

Regime.—The total accumulation volume of a glacier during one accumulation season and its gross ablation volume during the following ablation season constitute the regime, or material balance, of a glacier. If accumulation exceeds ablation, the regime is positive. The budget year for a glacier starts with the autumn accumulation, usually in September or October, and the sums on the balance sheet are expressed in cubic meters per square kilometer. The sum of total accumulation and total ablation is the glacier's balance-sheet total. Regime diagrams, consisting of a net ablation curve and an accumulatian curve for areas at specified altitude intervals, show at a glance the glacier's state of health. Climatological conditions in May and September exert particular influence on a glacier's regime; for lengthening or shortening of the ablation season has more effect on total ablation than has any other change. Temperature variation appears to be the basic and decisive

factor controlling changes in ablation. Furthermore, changes in ablation have greater influence on glacier regime than do changes in accumulation; at least, this is true on the Fourteenth of July and Hoffell glaciers.

Assembled data on a number of glacier regimes show that accumulation plus ablation in cubic centimeters of water per unit area at the firn limit is roughly the same as the average for the whole glacier. Thus a glacier's balance-sheet total equals the accumulation plus the ablation at the firn limit multiplied by the area of the glacier. If this relation holds in general, then observations to establish the total regime can be made at the firn limit. Meteorological observations at the firn limit also provide data on the climatic elements most effective in influencing a glacier's state of health.

Rate of movement.—Lines joining points of equal average rate of movement on a glacier's surface are "isotachytes," and isotachyte maps of several glaciers are presented. Admittedly, some of these are crude approximations, that of the Fourteenth of July Glacier, for instance, being based on only seventeen known points. Even so, the maps present principles of ice movement in striking fashion.

On Styggedal Glacier the rate of movement in the ablation area is 3–21 per cent greater in summer than in winter, and the difference is least when the rate is greatest. Great variations in movement of the Hoffell Glacier in different years and from place to place lead to the suggestion that waves of different velocity of movement proceed from the firn area to the glacier tongue. The rate of movement is to a large extent an expression of the amount of the glacier's regime or, more properly, the size of its balance-sheet total. If the latter is large, movement is relatively fast, even though the glacier may be receding, for much ice must be transferred from the accumulation area to the ablation area.

Classification of glaciers.—Classifying glaciers has long been an unsatisfactory business, and Ahlmann faces squarely up to the problem by advocating three separate classifications, namely, morphological (external features), dynamic (degree of activity), and geophysical (temperature and physical properties of the firn and ice). His morphological classification includes the major elements of earlier classifications and recognizes essentially three major divisions; ice sheets and caps, confined glaciers, and piedmont glaciers and related types. However, this arrangement is put on a more precise basis than in previous morphological classifications by the construction of percentile area distribution diagrams or curves for each of the principal types. These diagrams appear to be reasonably distinctive and should be a considerable aid in placing a glacier in its proper pigeonhole.

On a dynamic basis, glaciers are classified as active, passive, or dead. The activity or passivity of a glacier depends upon its depth, speed of flow, and balance sheet. The rate of movement of an active glacier is large in relation to its size and slope and that of a passive glacier is small in the same relations. Even a receding glacier may be classified as active if it has a large amount of ice to move from the accumulation area to the ablation area and is flowing at a rapid rate to accomplish this task. Glaciers on the south side of Vatnajökull have long had a negative balance and are consequently receding, but they lie in an area of intense glacial energy and are properly classed as active.

The definition of dynamically dead ice is as follows. "When the negative regime has continued until the movement of the remaining ice is no longer caused by the transfer of material from an accumulation area, but is determined only by the slope of the bed, the ice is said to be dead." This statement might be modified or extended to include Demorest's concept of pressure-controlled ice flowage, which, along with gravity flow, was used by Matthes as a basis for a different dynamic classification of glaciers. Ahlmann also observes that ice is dead from a climatological viewpoint when it no longer receives nourishment from an accumulation excess above a firn line but exists on its bulk alone. In a large glacier, several thousand years may elapse between climatic and dynamic deaths.

The geophysical classification is well known from Ahlmann's earlier writings and distinguishes temperate glaciers and polar glaciers with a subdivision into high-polar and subpolar in the last category. These distinctions are made largely on temperature regimen and physical condition of the firn and ice.

Variations of glaciers.—The glaciers investigated have been receding and shrinking because of a long-continued climatic amelioration, considerably intensified in the past decade. The present rapid, partly catastrophic, shrinkage stands out as the last stage in a decline which set in following the maximum advance of these glaciers within historical time. This advance may also have been the maximum within post-

glacial time. Throughout much of the North Atlantic region corresponding double-end moraines mark this maximum ice extent. This position has been reached in different areas at one or the other of the several "Hochstand" periods widely recognized as occurring (*a*) early in the seventeenth century, (*b*) about the middle of the eighteenth century, (*c*) early in the nineteenth century, and (*d*) about the middle of the nineteenth century. In Spitsbergen, at least, it is also recognized that the glaciers shrank to a fraction of their present size during the postglacial warm, dry period and a corresponding shrinkage is believed to have occurred in other areas. Similar behavior has long been recognized in Alaska. American glaciologists will be interested in a reference to work (unpublished?) by A. Hamberg in which he suggested as early as 1896 that the present glaciers of the Kvikkjokk district, Sweden, had probably been regenerated after the postglacial warm, dry period. This, of course, parallels Matthes' "Little Ice Age" in the Sierra Nevada.

Thorarinsson concludes from extended analysis of the literature that marginal variations of glaciers in recent centuries have in all probability been more or less concurrent all over the globe and, further, that the "Hochstand" in the middle of the nineteenth century has been demonstrated in most glacier districts of the world. The significance of such a conclusion is obvious with respect to any theory of glaciation requiring alternate ice ages in the Northern and Southern hemispheres. Although present glacier deterioration appears to be world-wide, the rise of sea level is so slow that the great Greenland and Antarctic ice sheets cannot be melting as rapidly as most of the smaller glaciers are, according to Ahlmann.

The present climatic amelioration in the North Atlantic features a rise in temperature of both sea and air. Meteorological records in Norway and Svalbard show that the temperature has increased incomparably more since the middle of the nineteenth century than at any other time in the last two hundred years. In general, the winter temperature has risen the most. As previously shown, the recession and shrinkage of glaciers around the North Atlantic is due mainly to increased ablation, which, in turn, is attributed chiefly to a rise in temperature. This temperature change has been most effective through lengthening of the ablation season in spring and autumn. The "recent climatic improvement" seems to have been brought about largely by an intensification of atmospheric circulation which produces an increased supply of warm air from the south.

The reader must guard against being lulled into a false sense of accuracy and precision by Ahlmann's many impressive diagrams and tables. It is clear that the author has no delusions on this score, but apparently he does feel that the magnitude of the sums involved reduces the errors to insignificance. Occasionally a more specific demonstration that this is true would be desirable and may possibly be supplied by reference to more complete treatments by Ahlmann and his associates in the various volumes of the *Geografiska Annaler*. Literally hundreds, if not thousands, of pages in that journal are devoted to the matters treated here in abbreviated form. More specific reference to that extensive literature would help the interested student in obtaining further information on matters barely mentioned in this summary. For instance, one would like to know how the thickness of ice in a glacier is calculated from its speed of movement, amount of ablation, and distance from the margin (p. 57). Further elucidation of the glacial limit and isoglacihypse lines might be furnished by a reference to the pertinent literature. In addition, such statements as "The ablation is now (*in the fall*) largely or wholly determined by temperature conduction," which are not obvious in themselves, might be more readily acceptable if reference to a detailed treatment were given.

The above are minor points, obviously arising chiefly from the necessary brevity of treatment. This work is truly a fine contribution, and the English-reading geologist can be grateful to Dr. Ahlmann for his internationalism and ubiquity in the field of languages as well as for his sterling scientific performance.

R. P. S.

An Introduction to the Theory of Seismology. By K. E. BULLEN. Cambridge: At the University Press; New York: Macmillan Co., 1947. Pp. xiii+263; figs. 43. $4.00.

The legitimate object of any technical writing is the transfer of information from one who knows more about a given subject to one who knows less. Accordingly, one of the first problems to which an author should direct his attention is to define the audience to whom he intends to address himself and then to formulate

his presentation in such a manner that the majority of that audience is able to follow without excessive difficulty what is being said. Within the chosen framework and at the selected level of technicality, the treatise should be as nearly as possible logically self-sufficient and complete.

The present book is a treatise on the theory of seismology, and its author is professor of applied mathematics at the University of Melbourne and a former student and research associate of Professor Harold Jeffreys, of Cambridge University. The audience to whom the book is addressed is said to comprise "observatory seismologists" and "university undergraduates who are interested in seismology as a branch of applied mathematics." This, in effect, constitutes two separate audiences, whose needs, interests, and backgrounds are somewhat divergent; and the one which appears to have received the greatest attention is the latter, namely, the university students of applied mathematics.

The logical outline of the book is good. It begins with the theory of elasticity and proceeds to a discussion of vibrations and waves and thence to body and surface elastic waves. Then follow applications to seismology. This includes the theory of seismic rays in a spherically stratified earth model, and the amplitudes of the surface motion due to seismic waves. Following this is a brief development of the theory of the seismograph. Then the construction of travel-time curves is discussed, and deductions from seismic data are made concerning the layering and physical properties of the interior of the earth. Final chapters deal with the occurrence of earthquakes and miscellaneous seismic topics.

One of the severest tests of the book concerns the manner in which the various theoretical and observational considerations and data have been brought together to disclose the nature of the earth's interior. The handling of this problem has been particularly well done. The author has displayed great skill in deriving the maximum unambiguous information concerning the earth's interior and has exercised commendable restraint in not letting his deductions transgress his data.

The weakest aspect of the book, in this reviewer's opinion, is the somewhat inhomogeneous manner in which different parts are written and its failure to meet the needs of its potential readers. The chapters on elasticity and wave theory in particular are much too tersely written to be read without excessive labor and expenditure of time by any but accomplished students of applied mathematics. Since paper is one of the cheapest of commodities and human time one of the dearest, this waste of the reader's time, which could easily have been avoided by expanding the development into a more readily intelligible form, does not appear to be justified. This comment is even more pertinent with respect to repeated instances wherein important equations are stated without derivation and it is remarked that their derivation is left as an exercise for the reader.

Since the book is not properly self-sufficient, it would have been helpful if citations of significant original literature had been given. The citations given are limited mostly to a dozen or so general treatises, only one of which (*Handbuch der Geophysik*, Vol. IV) is in a language other than English. References (without literature citations) to the work of individuals occur throughout the book, but almost every alternate reference is to Jeffreys. From this one finds it difficult to escape the impression that nearly all the world's seismic researches have been published in English and that, of these, about half have been the work of Jeffreys.

To summarize: While this book is unquestionably a valuable addition to one's reference library on theoretical seismology, it falls somewhat short, for reasons given above, of adequately meeting the needs of the audience to whom it is addressed, and hence it fails to achieve the status of the outstanding text that it otherwise might have been. It is to be hoped that in future editions the book will be considerably expanded with these considerations in view.

M. KING HUBBERT

Bulletin of the Oceanographical Institute of Taiwan, nos. 1–3 (November, 1946; April, 1947; and June, 1947). In co-operation with the National Taiwan University and Institut de Biologie de Shanghai. Taipei, Taiwan, China: Oceanographical Institute of Taiwan.

Taiwan, also known as Formosa, is the home of a newly established institute of oceanography. Founded by the Chinese government following World War II, the institute has issued three numbers of the *Bulletin*. These, dated November, 1946; April, 1947; and June, 1947, contain the following papers:

No. 1.— (1) "Observations on the Coral Reefs

of Penghu Islands, Taiwan," by T.Y.H. Ma, I. Hayasaka, and S. Kawaguchi; and (2) "On Fossil and Recent Brachiopods of Formosa," by I. Hayasaka.

No. 2.—"Submarine Valleys around the Southern Part of Taiwan and Their Geological Significance," by Ting Ying H. Ma; (2) "The Cause of Uplift of Eastern Asiatic Continent during the Middle Pleistocene Time," by Ting Ying H. Ma; (3) "Is There Any Particular Feature in the Marine Neogene Fauna of Taiwan?" by Ichirô Hayasaka; and (4) "Marine Molluscan Fauna from the Tashufang District, Kaohsiunghsien," by Sueo Kaneko.

No. 3.—(1) "The Upheaval Movement in Eastern Asia at the End of the Tertiary and the Cause Thereof," by Ting Ying H. Ma; and (2) "On the Formosan Spear-Fishes," by Kyosuke Hirasaka and Hiroshi Nakamura.

CHING CHANG WOO

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The *Acta Geologica Taiwanica* is sponsored by the Department of Geology of the National Taiwan University, under the chairmanship of Dr. Ting Ying H. Ma. The first issue of this new publication contains three papers: (1) "Amount of Emergence of Eastern Asia during the Middle Pleistocene Time," by Ting Ying H. Ma; (2) "A Permian Cephalopod Faunule from Chêchiang Province, China," by Ichirô Hayasaka; and (3) "Fossil Species of *Clypeaster* from Taiwan," by Ichirô Hayasaka and Akira Morishita.

CHING CHANG WOO

Historical Geology. By RUSSELL C. HUSSEY. 2d ed. New York and London: McGraw-Hill Book Co., Inc., 1947. Pp. ix+465; figs. 322. $3.75.

Historical geology is so large a subject and includes so many fields of study that any treatment of it in a single text must necessarily be elementary. Professor Hussey was apparently well aware of this when he wrote *Historical Geology*.

The introductory chapters cover such topics as the origin of the earth, the geologic time scale, superposition, stratigraphic correlation, and paleogeography. The closing chapter deals with the geologic history of man. The body of the text is subdivided on the basis of geological time. The Archeozoic and Proterozoic eras are covered in a chapter each. The Paleozoic and Mesozoic eras are treated on the basis of one chapter for each period, and the Cenozoic era is allotted one chapter for the Tertiary and one for the Pleistocene.

The new edition is somewhat shortened by the omission of the appendix, which contained a short résumé of the phyla of the plant and animal kingdoms. The remainder of the text has been changed in several minor respects. The order of the introductory chapters has been reversed: the chapters on evolution and paleontology are placed after those on physical geology and stratigraphy. A few illustrations have been added. The most noteworthy changes in the new edition are the omission of the tables of formations for the various periods and the addition of generalized sections for several of the periods.

In several cases the author presents but one side of questions which most geologists consider far from settled. An example of this is the discussion of Gondwana Land, a hypothetical land bridge which crosses a present ocean basin. Notwithstanding the possible affinities of geographically widely separated fossil forms, the physical evidence does not support the possibility of land bridges of this type. The conclusion of many of the workers in animal geography, such as R. Hesse, W. C. Allee, and W. D. Mathew, with regard to widely separated but closely related forms is that such land bridges are not necessary to account for the distribution of these animals.

The remarks on *Seymouria* are misleading because the position of this animal in classification is far from settled. In this and other cases the artificiality of classification and its primary use for convenience only should be pointed out to the reader.

The fact that controversy exists on these as well as other topics discussed by the author should be noted in the text. It is the opinion of the reviewer that unsettled questions should be pointed out to the student who is seeking an in

troduction to any science, so that he may become aware of the challenge and fascination that lie in exploring the unknown.

ROBERT L. MILLER

Fundamentals of Physical Science. By KONRAD BATES KRAUSKOPF. 2d ed. New York: McGraw-Hill Book Co., Inc., 1948. Pp. 676; figs. 330; pls. 1. $4.50.

This is one of the best—if not the best—textbooks available today for use in a general-education course in the physical sciences. It is of particular interest to geologists because of its emphasis on their science and on those parts of physics, chemistry, and astronomy necessary to a geologist.

In the Preface, Professor Krauskopf writes that his purpose is to deepen and enlarge the reader's interest in science by stressing the *methods* of scientific reasoning rather than their results. These few words give a formal solution, at least, to the problem of general education in science—the problem which is central in such a text as this one or in teaching a general-education course—of how best to give some broad understanding of science in a limited space or in a limited time.

The dangers are, on the one hand, of presenting too shallow a survey of the subject and, on the other hand, of going so deeply into one part that no picture of the whole is presented. Or, again, if insufficient attention is given to detailed facts, a complete discussion of methods becomes meaningless, while if the facts are heavily stressed, as in the traditional elementary textbook, then there is no time left to discuss the methods; in which case the casual student, upon finishing the book or course and passing the examination, promptly forgets the facts and is no better off than if he had never studied the subject.

Professor Krauskopf starts off by avoiding these dangers with a facility that is little short of genius: in the first hundred pages he covers positional astronomy, the elementary mathematics necessary to explain mechanics, mechanics itself and its application to the solar system—all in a brief, interesting, and understandable manner. The style can be paraphrased: "Here is what we see; how can we understand it?"

In Part II, "Matter and Energy," a well-integrated sequence of eight chapters carries the reader through kinetic molecular theory and chemical reactions to a fair understanding of the periodic table of the elements, based on the gross characteristics of matter. But in Part III, "The Structure of Matter," the style deteriorates somewhat toward the dogmatic, possibly because of the complex subject matter (atomic physics and physical chemistry) or possibly because the author feels that the reader is by now captivated and neither needs nor wants the naïve approach.

Chapter xxi, "The Atomic Nucleus," apparently re-written for this second edition to draw upon popular interest in the atomic bomb, is unfortunately sandwiched into the middle of a discussion which should lead without impediment to an explanation of chemical phenomena in terms of electric forces. The resulting confusion is most evident in the treatment of light: an introductory chapter, "Light Waves," following hard on "Electric Currents," is incomplete and is separated by two chapters ("X-Rays and Radioactivity" and "The Atomic Nucleus") from its sequel, "Radiation." After this oscillation the reader is plunged, all unawares, back into "Subatomic Chemistry."

Part IV, a section of one hundred pages, entitled "Fundamental Processes," covers ionic reactions, chemical equilibrium, carbon and silicon compounds, and colloids. This undisguised inflation of chemistry at the expense of physics would normally be undesirable in a well-balanced survey of the physical sciences. In this case it serves to support the extensive Part V, one hundred and sixty pages on the "Biography of the Earth."

The first chapter of Part V disposes of seismology somewhat too briefly in a mere three pages and includes no application of the earlier study of wave motion. The chapter on meteorology is a clear, elementary presentation, although the discussion of the general circulation of the atmosphere is not too convincing. The following chapters are entitled, "Rocks and Minerals," "Erosion and Sedimentation," "Volcanism and Diastrophism," "The Law of Uniform Change," "Interpreting the Rock Record," and "Earth History." The author is to be congratulated on fitting together so much so effectively in so small a space. The methods of the geologist are clearly stated, and some of the philosophy of the science is discussed in the chapter on uniformitarianism.

Following this, the last, sixty-page section on astrophysics could scarcely avoid being anti-

climactic. It is unfortunate that this section is so far separated from the chapters on light and that the treatment of Planck's law, Doppler effect, and double stars is not more quantitative. The existence of interstellar matter is completely ignored.

It would be possible to criticize many other omissions, of course, but a more basic criticism is that *Fundamentals of Physical Science* contains *too much* material for a sound, one-year course. The author himself suggests omitting several chapters in chemistry and geology, but this leaves the problem of supplying the connecting thread of the argument in the hands of the instructor. With this book as a text it will be appropriate to use geology as that thread; to adopt as the final goal in the student's mind the understanding of all the chemistry, physics, and astronomy which are necessary to understanding geology, as well as the geology itself.

The importance of books like this one can scarcely be overemphasized. It is true that the methods of science can be illustrated by the study of one science only—such as physics—but only an integrated picture of the physical world derived from the four sciences—astronomy, physics, chemistry, and geology—can show that *all* our physical experience fits into the pattern of scientific thought. Although it is not the final word on the subject, Krauskopf's *Fundamentals of Physical Science* represents a great advance in the integration of the physical sciences for elementary or nonspecialist readers.

THORNTON PAGE

Mineral Resources of Colorado. Prepared under the supervision of JOHN W. VANDERWILT. Denver: Colorado Mineral Resources Board, 1947. Pp. 547; pls. 34; figs. 10. $2.50.

This volume is divided into three parts: Part 1, "Metals, Nonmetals, and Fuels," by John W. Vanderwilt; Part 2, "Summaries of Mining Districts and Mineral Deposits," prepared by the United States Geological Survey under the general supervision of W. S. Burbank; and Part 3, "Investigations of Strategic Mineral Resources," by W. M. Traver, Jr., of the United States Bureau of Mines.

Part 1 contains a brief description of the general geology of Colorado, followed by a review, by counties, of the mineral production of the state. Numerous tables on production of minerals are included, as well as the principal references to each area. Vanderwilt has gathered together a huge amount of data, some of which was not in the literature, which should serve as a valuable reference.

Part 2 should be of interest to the mining geologist, teacher, and student. Emphasis has been placed on districts and, in some cases, metallogenetic provinces. Such a treatment makes the volume of much greater value than if it had been merely a geologic summary of the important mines of the state. It is to be regretted that some of the chief producing areas, such as Climax and Aspen, are not included in Part 2 and receive only the briefest treatment by Vanderwilt in Part 1. This may be due to the fact that these areas were recently reviewed in guidebooks of Colorado geology. In most cases the geologist most familiar with an area contributed the summary to this volume. The bulk of the work was done originally by the United States Geological Survey in co-operation with Colorado organizations.

Part 3 is brief (26 pp.) and consists principally of a listing of the mines examined by the United States Bureau of Mines as part of their strategic and critical minerals investigations. The sixteen exploratory projects of the bureau in Colorado are described.

This volume is primarily a review of Colorado mineral production and the geology of the important mining districts, including pertinent geologic literature. To the student and teacher, however, the value of the book is considerably increased by its emphasis in Part 2 on mining districts and, in some cases, on metallogenetic provinces. The wealth of descriptive material on Colorado mines has been digested and summarized to present the general geologic picture of the more important mining districts.

A. F. HAGNER

Symposium of Information Relative to Uses of Aerial Photographs by Geologists. Compiled by H. T. U. SMITH. (*Photogrammetric Engineering*, vol. 13, no. 4.) 1947.

The following papers are included, and after each a note is appended to indicate the contents. Much of the material has been printed previously, but some articles are new contributions. The symposium is particularly valuable inasmuch as it brings under one cover the im-

portant aspects of the use of aerial photographs in geology.

"Reconnaissance Mapping from Oblique Aerial Photographs without Ground Control." By JOHN L. RICH.

A method presented without use of Canadian grid or spot-location, but by use of transparent templates, so that position of faults, contacts, or position of strike and dip determinations can be approximately shown on the map.

"Geologic Interpretation of Trimetrigon Photographs, Northern Alaska." By SHERMAN A. WENGERD.

Recounts the procedure of geologic exploration of northern Alaska and the use and interpretation of trimetrigon photographs and the compilation of a structure map of the region from them.

"The Use of Aerial Photographs in Soil Mapping." By MARK BALDWIN, HOWARD M. SMITH, and HOWARD W. WHITLOCK.

General review of material printed in previous articles on the subject.

"Aerial Photographs in the Geological Study of Shore Features and Processes." By ROBERT S. DIETZ.

Reviews the kinds of shore features vividly shown on aerial photos, the use of photos in the study of waves, shore processes, and shore-line changes.

"Geological Mapping of the Ross Lake Area, Using Air Photographs." By Y. O. FORTIER.

The use of photos when no base map is available is described.

"Air Photographs in Geologic Mapping of Cordilleran Region, Western Canada." By A. H. LANG.

Remarks on preliminary office examination, planning traverses, plotting outcrops, obtaining rough idea of rocks, plotting geological boundaries, interpreting structure, extrapolating between traverses, physiographic data, and use in office compilation.

"Aerial Photographs as Aids in Stratigraphic Studies." By R. C. MOORE.

Detailed picture of strata is invaluable to guide stratigrapher to best outcrops and to help him study facies change.

"Aerial Photographs in Geology." By WILLIAM C. PUTNAM.

Review of general applications to geologic studies.

"Geologic Applications of Oblique Photography." By JOHN L. RICH.

Emphasizes use of obliques stereoscopically and explains briefly the use of obliques in mapmaking, how to determine dip, strike, and structure contours, and how to make a controlled aerial traverse.

"Advances in the Use of Air Survey by Mining Geologists." By G. W. ROONEY and W. S. LEVINGS.

Outlines use of photos in mining surveys in Canada, particularly the Beatty-Munro area, Ontario, and describes a course in aerial photos at the Colorado School of Mines.

"Use of Aerial Photographs in Glacial Geology." By F. T. THWAITES.

Emphasizes ability to study form of glacial deposits with aerial photos, which is of much value in mapping them.

"Multiplex Compilation of Geologic Maps." By JOHN D. STRABELL, JR.

An important contribution explaining the use of the multiplex in transferring geological data plotted on aerial photos to a base map, where considerable accuracy is required, as in mining districts.

"Aerial Photos in Geological Training." By H. T. U. SMITH.

Emphasizes advantages of aerial photos in teaching and as a new important technique in geologic mapping and research.

"A Method for Preparing Stereoscopic Aerial Photographs for Reproduction in Quantity." By JOHN L. RICH.

Where numerous copies of stereopairs are needed for classroom study with a lens-type stereoscope, a simple method of copying and printing standard 7×9 or 9×9 photos to the scale demanded for full coverage by the lens-type stereoscope is described.

A. J. EARDLEY

The Nature and Origin of Limestone Porosity. By RICHARD B. HOHLT. (*Colorado School of Mines Quarterly*, vol. 43, no. 4.) 1948. Pp. 48. $1.00.

Because of an increasing interest in the nature and origin of porosity in petroliferous limestones and dolomites, the author analyzes existing concepts and presents petrofabric data that bear on some of these concepts.

The first half of the paper is a discussion of primary porosity, "developed prior to or during lithification," and of secondary porosity, "developed subsequent to lithification." Because the economically important limestones are characterized by secondary porosity, primary porosity is only briefly outlined. Under secondary porosity "modifications of primary openings by solution action, jointing, fracturing and dolomitization" are considered.

In the latter half of the paper the author follows Bruno Sander, who first noted that the C-axes of calcite in limestones invariably lie in the plane of the bedding but with a random orientation within that plane, whereas dolomite crystals nearly always show a random orientation, although a few may show an orientation similar to that of limestone. Noting the diverse modes of origin of dolomite and the likelihood that dolomites with a preferred orientation are of primary origin, it is implied that limestones which have been dolomitized show a random orientation. The author concludes, other things being equal, that these newly formed dolomites "... may be expected to develop better solution porosity than a limestone . . ." due to this random orientation. The mechanism proposed for the formation of random orientation during dolomitization is that the newly formed dolomite crystals will orient themselves in response to zones of pressure release caused by the volume adjustment of Mg for Ca and selective leaching. In this study six petrofabric diagrams with corresponding photomicrographs of six different dolomites are included. However, these diagrams suggest a preferred orientation rather than the random orientation described. Five of the six diagrams show girdles containing point of maxima near the bedding plane, the sixth has a distinct girdle at a low angle to the bedding plane. There is little, if any, difference in the diagram by E. B. Knopf which was presented as an example of a "high degree of preferred orientation" of calcite and those described by the author as "random." On the whole, this study of porosity is stimulating, and the use of petrofabrics suggests a new approach to the basic problems of reservoir rocks.

F. A. JOHNSON, JR.

Emanuel Kayser's Abriss der Geologie, Vol. 2: *Historische Geologie* ("Outline of Geology—Historical Geology"). By ROLAND BRINKMANN. Stuttgart: Ferdinand Enke Verlag, 1948. Pp. vii+355; figs. 64; pls. 58. Unbound, M. 25; bound, M. 27.

For many decades, Kayser's textbook of geology has enjoyed a high reputation among European standard texts. Before the war the two-volume *Abriss* was supplemented by a four-volume *Lehrbuch*. Under extremely difficult conditions Brinkmann has succeeded in bringing the two-volume work up to date. (So far, the reviewer has not been able to obtain the first volume of this *Abriss*.) As Brinkmann states in the Preface, he has endeavored to supplant the dominantly stratigraphic presentation of historical geology in older editions by a closer elaboration of structural evolution of the earth's crust and paleogeography. The result is a most interesting, well-balanced, and clearly written representation of geologic history. The diverse, ever present mutual relations between areas of erosion and deposition, types of sediment, facies variations, volcanism, and crustal deformation are emphasized throughout the book, and the reader cannot but regret that the scope of the book prevented the author from giving much more detailed discussions.

After an introductory chapter on methods of measuring geologic time, the essential facts and interpretations of geologic history are presented. To compress the voluminous material, Brinkmann has added a great many new maps, showing paleogeographic elements, such as areas of contrasting facies, zones of regression, areas with related faunas or floras, shore lines of successive marine oscillations, marine-epicontinental versus marine-geosynclinal regions, isopach maps, partial reconstructions of climatic zones, distributions of particular faunas, etc. Other charts illustrate the stratigraphy of particularly interesting districts: pollen diagrams, progressive changes of selected families of organisms, climatic fluctuations, and sequences of earth events, similar to those employed by Von Bubnoff, Reed and Hollister, and others. As the

book will be used for the most part by geologists in Europe, it is understandable that this continent receives more attention than others. Moreover, the availability of particularly complete data for Europe adds to the reliability of most of these charts.

Eight large, folded plates correlate the stratigraphy of the Ordovician through Carboniferous and Triassic through Tertiary systems. Other periods are represented on smaller charts. They have all been worked out with great care, and much non-European literature up to the beginning of the war has been incorporated. At the end of each chapter are found selected references. They are well chosen, but the reviewer would like to see at least twice as many. If they were printed in small type and in consecutive lines, they would not increase the length of the chapters but would be of great aid as short cuts in our exceedingly scattered literature.

The book ends with a ten-page review of organic evolution, as well as the inorganic evolution of the earth's crust from the pre-Cambrian to the present. The book has a comprehensive index of 36 pages and is practically free from typographic errors. Reproduction of fossil plates and typography are very satisfactory, and the publication of this book is another gratifying sign that standards of technical European books remain high, despite the numerous complications of the postwar situation.

R. B.

Journal of Petroleum Technology, vol. 1, no. 1 (January, 1949). Dallas, Texas: American Institute of Mining and Metallurgical Engineers. $8.00 in Western Hemisphere, $9.00 elsewhere ($4.00 to A.I.M.E. members).

This journal is the official monthly publication of the Petroleum Branch of the American Institute of Mining and Metallurgical Engineers. It, together with its two sister-journals, *Mining Engineering* and *Journal of Metals*, supersedes the old *Mining and Metallurgy* as well as the four "technologies" (*Petroleum Technology, Mining Technology, Coal Technology,* and *Metals Technology*).

In the *Journal of Petroleum Technology* are found those items of special interest to petroleum geologists and engineers, items of Institute-wide character (which appear in all three of the new journals of the Institute), and the technical papers on petroleum geology and engineering which formerly appeared only in *Petroleum Technology*.

The segregation of subject matter into these three specialist journals should make each a more valuable publication for those who are specialists in the three respective fields. The style and format of the first issue of the *Journal of Petroleum Technology* is strongly reminiscent of the parent-journal from which it comes.

F. J. P.

International Rules of Botanical Nomenclature. Compiled by H. W. CAMP, H. W. RICKETT, and C. A. WEATHERBY. Waltham, Mass.: Chronica Botanica Co.; New York: Stechert-Hafner, Inc., 1947. Pp. 120. $3.50.

This volume is the result of action of the Committee on Nomenclature of the American Society of Plant Taxonomists. It represents a compilation "of what already has been acted upon favorably or authorized at official sessions of the Subsection for Nomenclature of the more recent International Botanical Congresses, together with certain items pertinent to the work of plant taxonomists and which legitimately should be included in such a volume."

As stated by the compilers, the volume is not to be construed as an official edition. It has been taken from original sources, and every effort has been made to avoid the introduction of errors and to interpret certain ambiguities as carefully as possible. The format of the third (1935) edition of the *Rules of Botanical Nomenclature* is followed throughout much of the volume with, however, rearrangement in the Appendix containing the conserved generic names.

The purposes of the volume are set forth in a short preface from which the material of the first two paragraphs of this review have been quoted or paraphrased. The book is attractively composed, and the typography is excellent. A utilitarian index that adds greatly to the ease of use of the volume has been included. Botanists and paleobotanists should find the publication of great service in their taxonomic studies.

E. C. O.

Submarine Geology. By FRANCIS P. SHEPARD. New York: Harper & Bros., 1948. Pp. vii+348. $6.00.

Submarine Geology carries the reader from the coasts and beaches, across the submerged continental shelves, and down their slopes to oceanic depths. He gains a comprehensive view of submarine topography and structure, as well as of the makeup of the present-day sea bottom. The author has incorporated the most recent data from all sources which this branch of oceanography has produced in its rapid development of the past few years. Much of the information in the chapters on the dynamics of beaches, the classification of coasts, the sediments of the continental shelves and slopes, and submarine canyons, as well as of the deeper ocean basins, stems from his own investigations.

The stratigrapher will do well to consider the sedimentary evidence presented in chapter vi. To those accustomed to thinking of an orderly gradation, from coarse to fine material, in an offshore direction and of uniform deposition over large areas, the patchy distribution of the sediments on the shelf will come as a surprise. Furthermore, the lateral transitions from one type of sediment to another are commonly abrupt. However, it should be remembered that these sediments are largely deposited in major oceans and that, at present, no water bodies really comparable to the epeiric seas of the past are known.

The origin and structures of continental shelves and slopes has always provoked speculation, and several alternative explanations are presented by Shepard, who arrives at the conclusion that the evidence points to a complex origin. A valid distinction is drawn between shelves off glaciated and off nonglaciated regions on the basis of topography and bottom sediments.

Atolls, guyots, and submarine canyons are cited as possible evidence of deep, world-wide submergence following a period of emergence. The reader is left with little doubt that the canyon problem is still unsettled, in spite of the author's strongly stated position; but there can be no questioning of the facts presented, many of which were obtained from the author's own field experience. Indeed, the diametrically opposed arguments that have been evoked whenever the question of origin is discussed should indicate to the impartial observer that, even at the present writing, the problem is far from settled. Witness the controversy between Kuenen, now the chief advocate of erosion by the muddy suspension current, and Shepard within the last year. However, the authors of the many hypotheses proposed to explain these puzzling gorges have one thing in common. Each one appeals to factors decidedly out of the ordinary, set forth in one single, comprehensive theory. Perhaps this appeal to the cataclysmic may eventually prove justified; but in this connection, in view of the somewhat dogmatic arguments advanced, a few lines from *Troilus and Cressida*, brought to my attention by H. W. Menard, Jr., seem apt to the still skeptical:

> Take but degree away, untune that string,
> And hark what discord follows. Each thing meets
> in mere oppugnancy. The bounded waters
> Should lift their bosoms higher than the shores
> And make a sop of all this solid globe.

Taken as a whole, *Submarine Geology* is an excellent exposition of the accumulated facts which are set forward without bias in the opening sections of each chapter. Shepard gives his preferred explanations of them, but there is nothing to prevent the questioning reader from placing his own interpretations upon them.

H. C. STETSON

The Permo-Triassic Formations. By R. L. SHERLOCK. London: Hutchison Scientific and Technical Publications, 1947. Pp. 367; figs. 15.

R. L. Sherlock has made a vigorous attack upon the problem of late Paleozoic and early Mesozoic time in this challenging treatment of the Permo-Triassic formations. In the concluding section of the book he recommends that recognition of the Permian system be abandoned in favor of association of the early Permian with the Carboniferous and the late Permian with the Triassic. While ingeniously argued and in many respects strongly documented, this proposal is likely to find few converts; for the inertia resulting from the weight of usage will be difficult to overcome. Although the proposal is stated in the closing section of the book, it is a theme which runs through many other parts and the framework upon which the book has been constructed. In some respects, however, this aspect of the work is less important than others, for divisions of geological time are, at best, arbitrary and are natural only in terms of the criteria which must be called upon to subdivide the continuum of

time into workable units. For this reason recognition of the Permian system, or any other system, must depend in large part upon practicality and usage. Arguments similar to those advanced against acceptance of the Permian can be raised to a greater or lesser degree for essentially every geological system; and, in view of the historical development of the geological timetable, the nature of the rock record, and the criteria available for differentiation of various major rock units, this seems inevitable.

Part I of this volume is concerned with several items pertinent to a study of the Permo-Triassic—the origin of red beds, climatic conditions, geography, paleontology, and, perhaps somewhat out of place in this volume, economic products. Red beds are considered to be predominantly continental in origin and, in most instances at least, indicators of semiarid or arid climates at the time of deposition. Although the author does not believe them to be infallible indicators of climate, he does cite red beds as supporting evidence of aridity. The rather impressive array of data which suggest that red beds may have originated under a wide variety of circumstances, whenever sufficient iron was present under appropriate oxidizing conditions prior to, during, or after deposition is not, perhaps, stressed sufficiently.

In the chapter dealing with "Climatic Conditions of the Permo-Trias," considerable space is devoted to the concept of "continental drift." In the course of the discussion the writer states: "We must conclude that the fact of 'Continental Drift' has been proved, although the mechanisms by which it was effected and the detailed results are still matters open for discussion." The considerations of climate are, of course flavored by this conclusion. The arguments carry over into the chapter on "Geography of the Permo-Trias." Aside from the paleogeographic picture, which differs sharply from that presented in most North American literature, and illuminating discussions of local Permian and Triassic conditions, some insight into the thinking of the author on the general problems of division of geological time is provided by this chapter. He does not admit the possibility of time intervals between periods, a conclusion certainly logical from a theoretical standpoint but most difficult to apply. Later in the book and bearing upon this point, he notes that the type Permian section in Russia is very incomplete and thus not suitable as a type section for a system. Completeness, of course, can be only a relative criterion, since no type section of any system records all the time of the period and some systems even lack well-defined type sections. Subsequently, however, the author uses as partial justification for the assignment of the late Permian to the Triassic the evidence of a "break" between lower and upper Permian beds. It is not entirely clear whether or not the faunal and floral discontinuities at this break are thought to imply a time interval. Sherlock's philosophy of the bases for division of geological time is not stated precisely in the volume, and efforts to reconstruct it lead to somewhat contradictory conclusions.

The chapter on "Paleontology" gives a short summary of the invertebrates, vertebrates, and plants of the two periods. For the purposes of this book the summary is satisfactory, although parts of it are rather out of date. Further discussion of the meaning of the various groups of organisms is given in later chapters. Two statements in the chapter are of particular interest. It is said that plants and animals are rarely fossilized in the same beds; hence correlation of floras and faunas are difficult. This is true so far as invertebrates and plants are concerned, but it is not the case for plants and vertebrates, which occur together under a wide variety of circumstances. The second statement is to the effect that in cases where the evidence from plants and that from animals is conflicting, it is better to give greater weight to the evidence from animals for practical reasons. This emphasizes the arbitrary nature of the criteria, for it is not infrequently true that the position of a major break in time will be determined quite differently if the plants or the animals are used alone. The dependency of a "natural" system upon the weighting of the criteria may, indeed, be very strong.

Part II deals with the "Stratigraphy of the Permo-Trias." In many respects this section, which comprises 157 pages, is the most important in the book. Treatment is by geographic units, and discussions, which draw heavily on the cited works of many writers, are moderately detailed. A compendium of information on the Permo-Trias is thus made available for the first time. The section presents the background for Section III on "The Classification of the Permo-Trias." The keynote to this section is set by the writer as follows: "The root of the problem of the Permian System lies in the fact that no one

has succeeded in determining a Period of time that is not represented by either Carboniferous or Triassic strata. In other words, all strata that have been called Permian may belong equally well to either the Carboniferous or the Triassic Systems." The arguments in this section are detailed and become as involved as the first quoted sentence is confusing. Few will disagree with the statement that the transition from the Carboniferous to the Permian is not clearly defined at many places either physically or biologically or that it is difficult to delimit the base of the Triassic in many instances. On the other hand, it seems adequately demonstrated, although the author seems to question it, that rocks were deposited between the times designated by established usage, varied as this may be in details, as Carboniferous, on the one hand, and Triassic, on the other.

Throughout the section, evidence of the twofold nature of the Permian is stressed, and the association of the earlier part with the Carboniferous and the later part with the Triassic is emphasized. The faunal and floral evidence for separating the lower and upper Permian beds depends in part on conflicting opinions and is not convincing. Evidence for relating the lower Permian beds to those of the Carboniferous is, however, strong. It is evident that geologists are faced with a difficult problem in developing a logical and practical division of the late Paleozoic and the early Mesozoic. Sherlock's proposal offers a solution based on fairly strong evidence so far as the criteria admitted for consideration are concerned. How much actual gain would come from adoption of the scheme is open to question. The sequence of beds in a given area and correlation of physical and biological events in time throughout the world are of utmost importance to the geologist in his task of reconstructing the history of the world. Any generally accepted series of time units can supply a frame of reference for this work as long as it is practical. There is serious doubt that Sherlock's plan is more workable the world over than is the one now generally in use in which the Permian system is recognized.

E. C. O.

Physiography of the Canadian Cordillera, with Special Reference to the Area North of the Fifty-fifth Parallel. By H. S. BOSTECK. (Canada Department of Mines and Resources, Geological Survey Mem. 247.) Ottawa, 1948. Pp. 106; pls. 32; colored map of physiographic subdivisions. $0.25.

As the result of extensive air photography by the United States Army Air Forces and the Royal Canadian Air Force, the author is able to present for the first time a unified physiography of the entire Cordilleran region in Canada. The area north of the fifty-fifth parallel is treated in detail, while that to the south, which is much better known, is described only as far as necessary to indicate regional relations.

Most of the report is given over to naming, outlining, and describing the larger physiographic units. Their relations to geologic structure and glaciation are considered, and in many areas extensive erosion surfaces are noted. The lack of geomorphic interpretations emphasizes the fact that here is a geologic frontier wide open to detailed investigations. The thirty-two airplane photos are one of the most interesting features of the report.

L. H.

COMMUNICATIONS AND ANNOUNCEMENTS

ANNOUNCEMENT OF NEW JOURNAL

With the aid of a grant from the Swedish government, the Swedish Geophysical Society has recently initiated publication of a quarterly journal, *Tellus*. This journal is to be a medium of publication for original contributions, survey articles, and discussions in the field of geophysical sciences. Each issue will contain 48 to 64 pages, with a page size of 179×250 mm. All articles in the first issue were in English, although articles in the French and German languages will also be accepted. The initial issue contained an article on the geochronology of the deep ocean bed by H. Pettersson, meteorological papers by T. Bergeron, E. Palmen, A. Nyberg, and C. G. Rossby, and a paper by O. Rybeck on recent ionospheric measurements.

The subscription price is $6.00 per year. Communications relating to manuscripts and subscriptions may be addressed to the Editor of *Tellus*, Fridhemsgatan 9, Stockholm, Sweden.

ERRATUM

In the article, "Oriented Lakes of Northern Alaska," by Robert F. Black and William L. Barksdale, vol. 57, no. 2, the captions under plate 1, *A*, and 1, *B*, are reversed.

The photographs from which plates 1–4 of this article were made are used through the courtesy of the United States Army Air Forces.